The Duration and Wage Effects of Long-Term Unemployment Benefits: Evidence from Germany’s Hartz IV Reform∗

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Abstract

Displaced workers often exhaust their initial unemployment benefits. I analyze Germany’s 2005 Hartz IV reform, which reduced the generosity of long-term unemployment insurance (UI) available once short-term benefits run out. Using administrative data on UI claimants, I exploit cross-worker and cross-cohort heterogeneity in the timing of Hartz IV’s effective onset to estimate how long-term benefit reductions affect jobless durations, subsequent wages, and job characteristics. The hazard rate of reemployment rises steadily in the months before the cuts take effect, culminating in a much larger spike in job-finding at benefit exhaustion than was evident before the reform. My estimates imply that the new benefit schedule reduced the probability of experiencing a one-year jobless spell by 12.4 percent. Conditional on completed jobless duration, workers who accept jobs after exhausting short-term benefits earn 4 to 8 percent lower wages than they would have absent the reform. Averaging across completed durations, and accounting for offsetting wage gains due to shorter spells, I conclude that UI reform reduced mean reemployment wages by 1.9 percent. Hartz IV diverted claimants from low-paid “mini-jobs” that often supplement UI receipt. Net employment gains are driven by full-time jobs.

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1 Introduction

Many displaced workers exhaust their unemployment benefits before returning to work. This is especially true during recessions, when jobs are hardest to find (Schmieder et al., 2012). Rather than ceasing benefit payments altogether, many countries—including Germany, France, the United Kingdom, Austria, Sweden, and Spain—rely on two-tiered systems of unemployment insurance (UI) that combine generous time-limited benefits with more modest unemployment assistance thereafter (Esser et al., 2013). These long-term unemployment benefits loom especially large for workers at the greatest risk of experiencing lengthy jobless spells, which erode employment prospects, deplete savings, and impose fiscal externalities through transfer payments and foregone tax revenue (Kroft et al., 2013; Ganong and Noel, 2016; Nekoei and Weber, 2016). Yet despite the widespread use of two-tiered benefit schedules, and despite renewed interest in long-term unemployment in the wake of the Great Recession, little is known about how long-term UI benefits affect jobless durations and subsequent earnings.

This paper analyzes the employment and wage effects of Germany’s 2005 Hartz IV reform, a prominent and controversial measure that reduced long-term benefit levels for both new and incumbent UI claimants. On January 1, 2005, existing long-term UI recipients—who numbered 2.2 million and comprised 5.3 percent of the civilian labor force on the eve of reform (Figure 1)—were switched overnight to the new, typically lower, post-reform benefit level. Subsequent inflows into long-term UI were subject to the new rules upon exhausting their initial stream of short-term benefits. The lack of grandfathering for incumbent claimants, together with concurrent changes in labor market conditions and institutions, poses difficulties for some of the standard quasi-experimental methods that are used to evaluate UI reforms. To overcome these challenges, I exploit cross-worker and cross-cohort variation in the timing of Hartz IV’s effective onset—based on individual heterogeneity in the potential duration of short-term benefits—to identify the causal effects of policy-induced benefit cuts. Using administrative data on over 336,000 UI claims made by prime-age displaced workers during 2001–2005, I find that exposure to the reform increases the hazard rate of reemployment beginning several months before the cut takes effect. The rising hazard rate—indicative of forward-looking behavior on the part of jobseekers—culminates in a much larger spike in job-finding at benefit exhaustion than was evident before the reform. My preferred estimates imply that being subject to the post-reform benefit schedule reduces the likelihood of experiencing a one-year jobless spell by 12.4 percent.

I then analyze how benefit cuts affect the wages workers receive upon being reemployed. To motivate this analysis, Figure 2 plots the mean difference in log monthly earnings before and after UI as a function of completed jobless duration for workers in my estimation sample. Whereas earnings fall only slightly in the wake of brief jobless spells, spells lasting over a year are associated with wage declines on the order of 20 to 25 percent, with especially sharp drops after modal short-term exhaustion points. To assess whether Hartz IV has contributed to this pattern, I extend the empirical framework to estimate the effects of long-term benefit cuts on the wages paid in a worker’s first job after entering UI. As prior research has noted, these effects are theoretically ambiguous (Nekoei and Weber,
2016; Schmieder et al., 2016). On the one hand, benefit cuts may prompt workers to accept lower-paying jobs at a given point in time by worsening their outside options in the event of continued unemployment. Consistent with falling reservation wages, I find that workers who accept jobs after exhausting short-term benefits earn 4 to 8 percent lower wages than they would have absent the reform, conditional on their completed duration. On the other hand, if time out of work reduces earnings capacity through stigma or skill depreciation, then benefit cuts may actually mitigate wage declines by shortening jobless durations. Furthermore, since post-UI wages are only observed if a claimant finds work within the sample period, benefit cuts introduce a standard selection problem by influencing transitions into reemployment. To quantify the net wage effect accounting for this selection margin, I jointly estimate the impact of Hartz IV on jobless durations and reemployment wages. I then decompose the overall effect into three components: direct effects at a given duration, indirect effects associated with changes in duration, and a compositional effect due to changing selection into reemployment. The negative direct effect dominates: adjusting for compositional changes in the observable characteristics of successful jobseekers, I estimate that Hartz IV reduced mean reemployment wages by 1.9 percent.

To shed additional light on the mechanisms underlying these wage effects, I next develop a competing-risks framework to examine how Hartz IV affected transitions into different kinds of jobs. First, I show that net employment gains are mostly driven by full-time jobs. Although I do not observe hours worked, a bounding exercise using the part-time share of employment suggests that my wage results likely reflect reductions in hourly wages rather than shifts along the intensive margin of labor supply. Second, I distinguish transitions into new jobs from recalls to the previous employer and find large, positive effects on both hazards. Insofar as recalls amount to options that workers choose to execute—rather than “search” as traditionally conceived—the increased recall hazard supports my interpretation that reform-induced benefit cuts have worsened jobseekers’ outside options in the event of remaining out of work. Third, I track transitions into a legally defined class of low-paid, part-time jobs that figure prominently in the political debates surrounding Hartz IV. My preferred employment concept excludes these so-called “mini-jobs”, which often supplement UI receipt and hence are unlikely to represent true returns to gainful employment. Broadening the definition of reemployment, I find that Hartz IV diverted claimants from mini-jobs into jobs covered by social insurance. This result adds an important nuance to the received wisdom that Germany’s UI reforms have fueled the growth of mini-jobs.

This paper makes two main contributions. First, I conduct the first unified analysis of the effects of reductions in long-term UI benefit levels on jobless durations, subsequent wages, and job characteristics. Although an extensive literature has examined changes in the level and potential duration of short-term UI benefits, much less is known about the long-term benefits at the heart of my study. Generous, indefinite-duration benefits of the

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1 Building on earlier work by Shavell and Weiss (1979) and Hopenhayn and Nicolini (1997), Kolsrud et al. (2016) show that the optimal slope of the benefit schedule depends on how benefits paid at different durations affect incentives for job search. Applying a regression-kink design to Swedish data, they find that workers respond throughout their spells to changes in long-term benefit levels, but less so than they do to comparable changes in short-term benefits. I complement Kolsrud et al. by using a different design applied to a different policy change in a different institutional setting. Unlike their study, I also analyze the impact of long-term benefits on subsequent wages and job characteristics—outcomes with first-order welfare effects in the presence of fiscal externalities (Chetty, 2006).
kind that existed in pre-reform Germany may strongly impact search behavior among the unemployed, as these benefits insure against the tail risk of a long or permanent jobless spell. In an influential series of papers, Ljungqvist and Sargent (1998, 2008) attribute European economies’ persistently high levels of long-term unemployment to the prevalence of such generous long-term benefits. My hazard analysis confirms that cutting long-term benefits leads to economically significant increases in individual job-finding rates. My wage analysis, in turn, complements an active literature that finds conflicting effects of changes in short-term benefit generosity on reemployment wages and match quality (e.g., Card et al., 2007a; Schmieder et al., 2016; Nekoei and Weber, 2016). I find that reductions in long-term UI generosity have negative effects on net wages, echoing similar findings for short-term generosity obtained by Nekoei and Weber (2016) in their study of Austrian benefit extensions.

Second, I present the first quasi-experimental evaluation of the microeconomic effects of Hartz IV, one of the most prominent social insurance reforms in recent memory. Throughout Europe, Hartz IV—the centerpiece of a broader package of “Hartz reforms” enacted in the mid-2000s—has become a byword for policies designed to strengthen job-search incentives among the unemployed. But despite intense interest from both academics and policymakers, previous studies have not credibly isolated variation in exposure to Hartz IV, which was rolled out simultaneously and uniformly throughout Germany. Nagl and Weber (2014) find that claimants return to work faster after the reform than before it, but their estimates are identified by time-series variation and do not account for other labor market reforms enacted during this period. Engbom et al. (2015) show that wages among previously displaced workers fell sharply relative to those of non-displaced workers during and after the Hartz reforms, but their strategy hinges on a strong parallel trends assumption and, as they acknowledge, “cannot reliably identify which element of the reform package was responsible for its effects.” I overcome these identification challenges by isolating cross-worker variation in exposure to benefit cuts within UI entry cohorts.

A complementary literature in macroeconomics has calibrated search-and-matching models, often to pre-reform data, to simulate the aggregate impact of Hartz IV (Krause and Uhlig, 2012; Krebs and Scheffel, 2013; Launov and Wälde, 2013). These papers reach disparate conclusions about the impact of benefit cuts on steady-state unemployment, with estimates ranging from 0.1 to 2.8 percentage points. Although my paper is silent on general equilibrium mechanisms such as congestion externalities or job creation, credible estimates of the partial equilibrium effects are an important input into understanding Hartz IV’s aggregate impact. A back-of-the-envelope
calculation suggests that these partial equilibrium effects—if not augmented or offset by general equilibrium forces—may have lowered Germany’s steady-state unemployment rate by 0.9 percentage points. This calculation is merely illustrative: a careful reckoning of Hartz IV’s aggregate effects is beyond the scope of this paper.

The rest of the paper is structured as follows. Section 2 describes Germany’s UI system and explains how Hartz IV altered the benefit schedule. Section 3 uses a model of job search to motivate the empirical strategy. Section 4 describes the data and research design. Section 5 estimates the effects of benefit cuts on job-finding and jobless durations. Section 6 analyzes wages. Section 7 uses a competing-risks approach to distinguish among transitions into full-time, part-time, and mini-jobs, as well as between new jobs and recalls. Section 8 concludes.

2 Reform of the German UI System

Prior to 2005, Germany had a two-tiered UI system consisting of time-limited unemployment benefits (Arbeitslosengeld) which, when exhausted, could be followed by means-tested unemployment assistance (Arbeitslosenhilfe). I refer to these sequential benefit streams as “short-term” and “long-term” benefits, respectively. Hartz IV left short-term benefits unaltered. To establish an entitlement to short-term benefits, an individual must have worked at least 12 months over the preceding 3 years. In the event of job loss, eligible workers are entitled to benefits equal to 60 percent of their prior after-tax earnings (67 percent for claimants with dependent children), averaged over the past year or, if more favorable, two years. Benefit payments are not taxed, and claimants may work at most 15 hours per week under a net earnings disregard equal to the larger of \( A \) or 20 percent of prior earnings. The potential duration of short-term benefits, \( P \) (in months), is a step function that depends on age at claim initiation (\( a \)) and on months of covered work experience accrued over the preceding seven years (\( x \)):

\[
P = \min(\bar{P}(a), 2 \cdot \text{floor}(x/4)),
\] (2.1)

where \( \bar{P}(a) \) is an age-specific maximum duration. Hence 12 months of work experience yield 6 months of benefits, and every 4 months of additional work experience extend short-term duration by 2 months. For workers under 45, benefits cannot exceed 12 months. From age 45 onward, the maximum duration rises first to 18 months, then to 22 months (at age 47), then to 26 months (at age 52), and finally to 32 months (at age 57). Figure 3 summarizes the benefit accrual rules applicable to workers in my estimation sample (ages 25–54 at claim initiation). Although new entitlements established under these rules last at least 6 months, shorter potential durations are possible in cases of seasonal employment or the resumption of an unexhausted benefit claim. Seasonal workers who do not satisfy the normal eligibility criteria are entitled to 3 months of benefits if they have worked at least 6 months, or to 4 months of benefits for 8+ months of work. Furthermore, a claimant who exits UI without having used up her short-term benefits remains entitled to her remaining benefits in the event of a new job loss. If she establishes a fresh entitlement in the interim, she is entitled to 6 months of benefits plus her residual benefits from the previous entitlement, with the sum capped at the age-specific maximum. Despite these carryover provisions, potential duration (in days) can assume any integer value from 1 to the maximum.

\[5\]“Short-term” benefits can be quite lengthy, lasting up to 26 months for workers in my sample. Conversely, some workers transition to “long-term” benefits quite early in their spells. This terminology should not be confused with notions of “long-term unemployment” that depend only on duration since the start of unemployment, without regard to UI eligibility.

\[6\]Seasonal workers who do not satisfy the normal eligibility criteria are entitled to 3 months of benefits if they have worked at least 6 months, or 4 months of benefits for 8+ months of work. Furthermore, a claimant who exits UI without having used up her short-term benefits remains entitled to her remaining benefits in the event of a new job loss. If she establishes a fresh entitlement in the interim, she is entitled to 6 months of benefits plus her residual benefits from the previous entitlement, with the sum capped at the age-specific maximum. Due to these carryover provisions, potential duration (in days) can assume any integer value from 1 to the maximum.
the data section, plots the observed variation in potential benefit duration for claimants in my estimation sample.

Upon exhausting short-term benefits, a UI recipient could apply for long-term unemployment assistance. Unlike the initial benefit stream, long-term benefits were means-tested on the basis of household assets and income. For a worker passing the means test, the benefit level equaled 53 percent of former net earnings (57 percent with dependent children), with benefit payments falling one-for-one with spousal earnings. Long-term benefits were unlimited in duration, provided that claimants continued to satisfy the means test. Poor households could also apply for supplemental means-tested welfare to top off their UI benefits. The combination of a generous long-term UI benefit level—in many cases only slightly below the short-term level—and indefinite duration set Germany apart from its European neighbors (Wunsch, 2006). The UI caseload grew steadily in the early 2000s: by June 2004, 2.2 million workers were claiming long-term UI benefits, on top of another 1.7 million claiming short-term benefits (Figure 1). The growing fiscal burden, together with a widespread belief that the safety net was too generous, created political pressure for labor market reform.

In March 2002, the center-left government convened a commission led by former Volkswagen director Peter Hartz to recommend a reform package. The Hartz Commission’s report, released in August 2002, proposed a wide range of measures intended to put the unemployed back to work through a mixture of carrots and sticks. The first reform measures, which came into effect in January and April 2003 (Hartz I and II) and January 2004 (Hartz III), liberalized the temporary help sector, expanded favorable tax treatment for mini-jobs, provided start-up subsidies for entrepreneurs, and restructured the Federal Employment Agency.\textsuperscript{7} While these earlier measures may have had important effects in their own right, the Hartz IV overhaul of the UI system is widely regarded as the centerpiece of the entire reform package.\textsuperscript{8} Hartz IV was passed by the lower house of parliament in December 2003, confirmed by the upper house in July 2004, and implemented on January 1, 2005.

The crux of Hartz IV was to consolidate long-term unemployment assistance and welfare payments into a single, means-tested income-support program. In contrast to the old regime, long-term benefits are no longer indexed to prior wages. Instead, each long-term claimant now receives a standard monthly payment (equaling €345 in the West and €331 in the East in 2005, with slight increases in subsequent years), plus additional benefit payments for dependent spouses and children as well as assistance with rent and heating. To cushion the drop in benefit level, some long-term UI recipients were also eligible for a temporary supplemental payment.\textsuperscript{9} Means testing was tightened relative to pre-reform criteria, and changes in benefit levels were accompanied by tightening of job search requirements and sanctions for those who violated program rules.

Although the post-reform UI system was broadly less generous than its predecessor, some claimants saw

\textsuperscript{7} See Ebbinghaus and Eichhorst (2009) for a lengthier discussion of the overall reform package, which had many components. Tompson (2009) provides a detailed account of the political context.

\textsuperscript{8} Writing about the Hartz IV benefit cuts in \textit{The New York Times}, Landler (November 26, 2004) noted, “[E]conomists say these will be the most important measures in the whole package.”

\textsuperscript{9} The supplement lasted up to two years and was halved in the second year. Many workers did not qualify for the supplement, however, and those who did faced further step-downs as the supplement waned and then expired. I include this supplement in the benefit simulations described below and calculate reform-induced changes in potential household income just after short-term benefit exhaustion, when the supplement is maximized. See Appendix A for details.
their benefit levels—inclusive of supplemental payments and in-kind assistance with rent and utilities—go up after the reform.\footnote{Around the time of implementation, \textcite{bolll2005} estimated that one-sixth of long-term recipients would lose their long-term benefits entirely, but that roughly half of remaining claimants would receive a benefit increase. Bolll and Rudolph find, as do I, that benefit cuts among those facing cuts were generally larger in magnitude than benefit gains among those receiving gains.} To establish that Hartz IV had bite, and to gauge the “first-stage” impact of UI reform on net transfers to the unemployed, I adapt programmatic rules from the OECD Tax-Benefit Model to simulate the reform-induced change in household income for each claimant in my estimation sample (which is described in \textsections{4.1} and \text{4.2}). Some important inputs, notably household assets and spousal earnings, are not reported in the microdata. Appendix A describes my simulation algorithm, together with the imputations I perform to account for unreported inputs. I feed each claimant’s observed and imputed characteristics through the 2004 and 2005 tax and benefit rules. To account for any offsetting changes in other taxes and benefits—as other parts of the safety net may adjust to pick up part of the slack—I measure the financial impact of Hartz IV as the hypothetical change in post-exhaustion income that a household would experience as a result of switching from 2004 to 2005 rules:

\[
\%\Delta (\text{household income}) \equiv \frac{\Delta (\text{net household income after exhausting short-term UI, 2005 vs. 2004 rules})}{\text{net household income after exhausting short-term UI, 2004 rules}} \quad (2.2)
\]

Given the imputations needed to simulate net income in these data, this measure should be interpreted with caution. Nonetheless, it provides a useful window into how Hartz IV altered household income after benefit exhaustion.

Figure 5 plots the simulated distribution of reform-induced income changes for the claimants in my sample. About 80 percent of men and 70 percent of women incur apparent drops in (potential) post-exhaustion household income as a result of Hartz IV. The median claimant faces a prospective decline in net household income of about 5 percent, with nearly one-quarter of claimants confronting declines of over 10 percent. There is a large point mass near zero, mainly representing married claimants who are ineligible for long-term UI under both old and new rules by virtue of having a high-earning spouse. Fewer than 5 percent of claimants experience benefit gains exceeding 5 percent of prior net income. Several additional features of Hartz IV made long-term benefit receipt less attractive than these numbers imply. The reform included stricter means testing of household assets as well as stricter application of benefit sanctions for long-term UI recipients. Many claimants faced additional step-downs at lengthier durations, as the temporary supplement declined and then expired. On the whole, it appears that Hartz IV made long-term unemployment less appealing for most workers, with countervailing benefit increases generally modest and transitory. To simplify the exposition, I refer to reform-induced benefit changes as “cuts”, recognizing that a minority of claimants may have experienced slight gains instead.

Three additional aspects of Hartz IV warrant emphasis. First, incumbent claimants were not grandfathered in under the pre-reform rules. Effective January 1, 2005, anyone already claiming long-term benefits was immediately converted to the new, generally lower benefit level.\footnote{Legal challenges casting the benefit cuts as an illegal seizure of property were ultimately dismissed by the courts.} My research design in \textsection{4} is expressly designed to account for this policy feature. Second, the reform became publicly salient in July 2004. Shortly after final passage
of the reform act, the government mailed existing claimants a 16-page questionnaire meant to gauge their eligibility for long-term benefits under the new, stricter means test (Tompson, 2009). The questionnaire sparked considerable confusion and led to protests against Hartz IV in dozens of German cities (BBC News, August 17, 2004). The publicity surrounding these events helps rationalize the forward-looking behavioral responses I find throughout the paper.\(^{12}\) Third, because Hartz IV replaced a wage-indexed benefit with a flat, standardized payment, individuals with greater prior earnings tended to receive higher long-term benefit levels before 2005 but steeper cuts thereafter. Even if I could perfectly measure each claimant’s reform-induced benefit cut, variation in these cuts would therefore be correlated with prior earnings, which in turn may be correlated with responsiveness to a given benefit cut. My core identification strategy sidesteps this concern by relying on the timing of benefit changes rather than their simulated magnitude. I discuss heterogeneous benefit changes further in Sections 5.4 and 5.6.

UI claims initiated after February 1, 2006 were subject to additional policy changes, including tighter eligibility rules as well as reductions in potential short-term duration for older workers.\(^{13}\) Critically for my purposes, existing claims were not subject to these subsequent reforms. I restrict my sample to UI spells beginning prior to 2006, so that my analysis is not confounded by changes in short-term UI eligibility or duration.\(^{14}\)

### 3 Theoretical Framework

To clarify how reductions in long-term UI benefits might affect individual jobless durations and subsequent wages, I use a continuous-time job search model in the spirit of Mortensen (1977) to develop three predictions tailored to my empirical setting. First, a reduction in the long-term benefit level increases the reemployment hazard and decreases the reservation wage at all durations, as forward-looking agents react to future benefit cuts. Second, these behavioral responses approach zero in the limit as cuts lie increasingly far in the future. This limiting result offers a theoretical justification for my research design, which presumes that claimants facing far-off benefit cuts are a suitable reference group for counterfactuals in which these cuts do not occur at all. Third, although benefit cuts depress mean reemployment wages via lower reservation wages, this effect is at least partly offset by wage gains due to shorter durations.

Consider a displaced worker who searches for a job until reemployed. Search yields job offers at flow rate \(s\)\(^{12}\) Consistent with this timing, Google Trends shows a sharp uptick in searches for the terms “Hartz IV” and “Arbeitslosengeld” in the summer of 2004 (Appendix Figure 1). “Hartz IV” was also the 2004 German word of the year. As the chairman of Germany’s language association stated at the time, “No term has been used so often this year, at least in politics. Even someone who does not know about Mr. Hartz understands something by the term Hartz IV” (Frankfurter Allgemeine Zeitung, December 10, 2004, own translation). Although July 2004 was clearly an information event, attentive claimants may have suspected that benefit cuts were coming well before this date. I discuss the timing of anticipatory behavior in the context of a placebo exercise in Section 5.5 and Appendix B.

\(^{13}\) Under a deferred provision of Hartz III, the lookback period for establishing a UI entitlement was reduced from 3 years to 2 years; special provisions for seasonal workers were eliminated as well. Under the Labour Market Reform Act—a law adopted simultaneously with Hartz IV but technically separate from it—the cap on short-term duration fell to 12 months for workers ages 45–54 and to 18 months for workers 55 and over. These changes proved deeply unpopular and were partly reversed in 2008 (Lichter, 2016).

\(^{14}\) Dlugosz et al. (2014) show that, in the weeks preceding February 1, some workers strategically timed job losses so as to be covered under the old rules. Although the number of retimers was small, strategic retiming could lead to compositional changes that would threaten identification of Hartz IV’s effects. I show in Section 5.4 that my results are robust to either controlling for unobserved heterogeneity by quarter of UI entry or excluding claims initiated after July 2004, suggesting this is not a serious concern.
and entails disutility $\psi(s)$, where $\psi(\cdot)$ is convex and satisfies Inada-type conditions that ensure an interior optimum. Wage offers are drawn from a stationary continuous distribution $G(\cdot)$. Once accepted, a new job lasts forever. The worker receives flow utility of consumption $u(\cdot)$, discounted at rate $\delta > 0$. She cannot borrow or save.

In line with the empirical setting, I model UI as a two-tiered benefit schedule. The potential duration of short-term benefits is $P$. Letting $R \geq 0$ denote the remaining duration of benefits at a given point in time (so that $R = P$ at the beginning of the claim), UI benefits equal

$$b(R) = \begin{cases} \bar{b} & \text{if } R > 0 \\ \bar{b} & \text{if } R = 0 \end{cases} \quad \text{with } 0 < \bar{b} < \bar{b}. \tag{3.1}$$

The benefit step-down is the sole source of nonstationarity in this model, and $R$ is the sole state variable. Hartz IV operates by lowering $\bar{b}$, with effects varying as workers approach short-term benefit exhaustion.

Let $U(R)$ denote the indirect utility from being unemployed with $R$ months of benefits remaining, and let $J(w) = \frac{u(w)}{\delta}$ denote the indirect utility from being employed forever at wage $w$. As in Mortensen (1977), the optimal policy entails a cutoff strategy with reservation wage $\bar{w}$, so that $U(R)$ admits a Bellman representation:

$$\delta U(R) = \max_{s,w} u(b(R)) - \psi(s) + s(1 - G(\bar{w}))(E(J(w) \mid w \geq \bar{w}) - U(R)) - \dot{U}(R) \tag{3.2}$$

where $\dot{U}(R) \equiv \frac{dU(R)}{dR}$. The solution consists of policy functions $\bar{w}(R)$ and $s(R)$, denoting the reservation wage and search intensity chosen at each duration. The empirical hazard rate of reemployment is $\lambda(R) \equiv s(R)(1 - G(\bar{w}(R)))$, reflecting the need to first obtain a job offer and then accept it.

Claim.

(a) A long-term benefit cut increases search intensity and decreases the reservation wage throughout the unemployment spell. That is, $\frac{ds(R)}{db} < 0$ and $\frac{d\bar{w}(R)}{db} > 0$ for all $R \geq 0$. It follows immediately that $\frac{d\lambda(R)}{db} < 0$, so that a benefit cut increases the hazard rate of reemployment at all durations.

(b) These behavioral responses tend to zero for benefit cuts lying arbitrarily far in the future. That is, $\lim_{R \to \infty} \frac{ds(R)}{db} = \lim_{R \to \infty} \frac{d\bar{w}(R)}{db} = \lim_{R \to \infty} \frac{d\lambda(R)}{db} = 0$

(c) There are offsetting effects on mean accepted wages. Observed wages decline conditional on completed duration, but workers accept jobs at earlier durations when reservation wages are higher.

Part (a) is intuitive: benefit cuts make unemployment less attractive, and workers try harder to escape unemployment before exhausting short-term benefits. Part (b) reflects discounting: when $R$ is very large, search intensity and reservation wages asymptote to the values workers would choose if benefits remained perpetually at the elevated level $\bar{b}$. These limiting values are invariant to $\bar{b}$. Part (c) captures the ambiguous effect of UI generosity on

\[ \text{for many commonly used functional forms, these behavioral responses dampen monotonically with time remaining until the benefit cut. For constant relative risk aversion utility functions } u(c) = \frac{1}{\gamma}c^{\gamma}, \text{ a sufficient condition for the (log) reservation-wage response to dampen monotonically is that } \gamma \geq 0. \text{ This nests risk neutrality and log utility as special cases. For the hazard rate, the same result obtains under linear utility, quadratic search costs, and an offer distribution for which the pdf } g(w) \text{ is decreasing over its support.} \]
post-UI earnings, noted previously by Schmieder et al. (2016) and Nekoei and Weber (2016).

**Proof of (a).** There is no closed-form solution for the optimal policies without functional form assumptions, but I can characterize the solution using standard dynamic programming techniques. Given the Inada conditions on \( \psi(\cdot) \), an optimizing worker exhausts her short-term benefits with positive probability, so that \( U(R) \) is strictly increasing in \( b \) for all \( R \). Furthermore, the (weakly) declining benefit schedule ensures that \( U(R) \) is increasing in \( R \). The net present value of unemployment declines as a worker approaches benefit exhaustion and is constant thereafter.

The first-order conditions for the optimal reservation wage and search intensity are, respectively,

\[
J(w(R)) = U(R) \\
\psi'\left(s(R)\right) = (1 - G(w(R))) \left( E(J(w) \mid w \geq w) - U(R) \right)
\]

(3.3) \hspace{2cm} (3.4)

I next derive a useful intermediate expression showing how the value of unemployment responds to marginal changes in \( b \). After the exhaustion of short-term benefits, the Bellman simplifies to

\[
U(0) = \frac{u(b) - \psi(s(0)) + s(0)(1 - G(w(0)))E(J(w) \mid w \geq w(0))}{\delta + s(0)(1 - G(w(0)))}
\]

(3.5)

so that, invoking the envelope theorem,

\[
\frac{dU(0)}{db} = \frac{u'(b)}{\delta + s(0)(1 - G(w(0)))} > 0
\]

Using this property, and again applying the envelope theorem,

\[
\frac{dU(R)}{db} = \exp(-R) \Pr(\text{reach exhaustion}) \frac{dU(0)}{db}
\]

\[
= \exp(-R) \exp \left( - \int_0^R \lambda(x) dx \right) \frac{dU(0)}{db}
\]

\[
= \exp \left( - \int_0^R (\delta + \lambda(x)) dx \right) \frac{dU(0)}{db}
\]

(3.6)

This expression implies that \( \frac{d}{dR} \left( \frac{dU(R)}{db} \right) < 0 \). The intuition is straightforward: to first order, small changes in the long-term benefit level affect utility only once benefits are exhausted, and post-exhaustion utility is discounted at the effective rate \( \delta + \lambda(\cdot) \). When \( R \) is greater, the future is discounted more heavily due to both pure time preference and the increased likelihood of interim reemployment.

Next, applying the implicit function theorem to Equations 3.3 and 3.4, and using \( J(w) = \frac{u(w)}{\delta} \), yields

\[
\frac{dw(R)}{db} = \frac{\delta}{w'(w(R))} \frac{dU(R)}{db} > 0 \text{ for all } R
\]

(3.7)
The effect of a benefit cut on the hazard rate is immediate:

\[ \frac{d\lambda(R)}{db} = \frac{ds(R)}{db}(1 - G(w(R))) - s(R)g(w(R)) < 0 \text{ for all } R \]  

(3.9)

This establishes (a): long-term benefit cuts depress reservation wages and increase job-finding at all durations.

Proof of (b). To prove that behavioral responses limit to zero for far-off benefit cuts, consider a hypothetical benefit scheme that pays the generous amount \( b \) in perpetuity. This is a stationary problem with value \( U^* \) defined by

\[ \delta U^* = \max_{s,w} \left( u(b) - \psi(s) + s(1 - G(w)) \left( \mathbb{E}(J(w) \mid w \geq w) - U^* \right) \right), \]  

(3.10)

and with associated (stationary) policies \( s^* \) and \( w^* \) and associated hazard rate \( \lambda^* \equiv s^*(1 - G(w^*)) \).

A revealed-preference argument shows that \( U(R) \) limits to \( U^* \). Under the true benefit schedule \( b(R) \), let \( \tilde{U}(\cdot) \) denote the payoff from adopting strategies \( s^* \) and \( w^* \) for all \( R > 0 \) and then switching to the true optimal policies \( s(0) \) and \( w(0) \) after the benefit step-down. Since the flow payoffs and hazard rates that result coincide with those in the hypothetical problem until benefit exhaustion and with those in the true problem thereafter,

\[ U^* - \tilde{U}(R) = \exp(- (\delta + \lambda^*)R)(U^* - U(0)). \]  

(3.11)

This utility gap decays exponentially, so that \( \lim_{R \to \infty} \tilde{U}(R) = U^* \). But by revealed preference, \( \tilde{U}(R) \leq U(R) < U^* \) for all \( R \). Hence \( \lim_{R \to \infty} U(R) = U^* \).

Returning to the first-order conditions in Equations 3.3 and 3.4, take limits as \( R \to \infty \). By continuity of \( u, \psi, \) and \( G \), the policy functions approach well-defined limits: \( \lim_{R \to \infty} s(R) = s^* \) and \( \lim_{R \to \infty} w(R) = w^* \). Now take the limit as \( R \to \infty \) in Equations 3.7 and 3.8. By continuity, both expressions limit to zero, proving (b).

Proof of (c). Finally, consider how benefit cuts affect accepted wages, i.e. the wage measure observed by the econometrician. Let \( \bar{w}(R(D)) \equiv \mathbb{E}(w \mid w \geq w(R(D))) \) denote the average accepted wage among workers reemployed \( D \) months into their unemployment spell, with \( R(D) \equiv \max(0, P - D) \) denoting the remaining duration until benefit exhaustion at this time. Let \( f(D) \equiv \exp \left( - \int_0^D \lambda(x)dx \right) \lambda(R(D)) \) denote the pdf of completed jobless duration. It is instructive to write the mean accepted wage as a weighted average of duration-specific average wages:

\[ \mathbb{E}(w) = \int_0^\infty f(D)\bar{w}(R(D))dD. \]  

(3.12)

Differentiating with respect to \( b \),

\[ \frac{d\mathbb{E}(w)}{db} = \int_0^\infty f(D)\frac{d\bar{w}(R(D))}{db}dD + \int_0^\infty \frac{df(D)}{db}\bar{w}(R(D))dD. \]  

(3.13)
The first term is the reservation wage effect, which can be shown to equal

\[
\int_0^\infty f(D) \left\{ \frac{g(w(R(D)))}{1 - G(w(R(D)))} (\bar{w}(R(D)) - w(R(D))) \frac{dw(R(D))}{db} \right\} dD > 0.
\]

(3.14)

The second term is an indirect effect. Since benefit cuts increase job-finding at all durations, they shorten jobless durations in the sense of first-order stochastic dominance. This puts more weight on mean wages at short durations, when the reservation wage and hence \(\bar{w}(R(D))\) are higher. Thus the second term is negative, proving (c). \[16\]

In a more general model with duration-dependent earnings potential, the duration effect may be amplified because shorter durations lead to less stigma or loss of skills. An additional selection term emerges if, as in practice, workers are heterogeneous and not everyone is reemployed within the sample period. I pursue this idea in Section 6, where I decompose the net wage effect of Hartz IV into reservation wage, duration, and selection components.

4 Data and Research Design

4.1 Worker-level administrative data

To study the effects of UI reform on unemployed workers’ jobless durations and eventual wages, I use individual work histories drawn from Germany’s Integrated Employment Biographies, an administrative dataset that combines records on employment, unemployment, and benefit receipt. The source data are used by federal tax and benefit authorities for the assessment of social insurance contributions and for the calculation of UI benefits. I accessed two anonymized extracts under agreement with the data custodian, the Institute for Employment Research (IAB).

Most of my analysis relies on the IAB/IZA Administrative Evaluation Dataset (AED), a 4.7 percent random sample of all individuals who registered with the unemployment office anytime during 2001–2008 (Eberle and Schmucker, 2015). For each worker, I observe rich demographics (sex, year of birth, nationality, education, household structure, and district of residence), employment information (including average daily earnings, part-time/full-time status, establishment ID, and industry), and detailed information about periods of unemployment and UI receipt. These work and benefit histories span the years 1993–2010, and all spells are recorded at daily frequency. Since the AED is not representative of pre-2001 flows into unemployment, some parts of my analysis rely on the SIAB, a 2 percent random sample of all individuals who appear in the underlying data universe anytime during 1975–2010 (vom Berge et al., 2013). For each worker, the SIAB reports all of the data elements listed above.

These datasets have several limitations relevant to my analysis. First, I do not observe all of the inputs into the means-testing procedure that determines eligibility for long-term benefits under the old or new rules. Notable omissions include household assets and spousal earnings. These missing inputs make it impossible to know precisely how an individual’s net benefit schedule was changed by Hartz IV, and they preclude me from studying the effect of

\[16\] In a closely related model, Nekoei and Weber (2016) prove that marginal changes in the level of short-term benefits have indeterminate effects on reemployment wages: under different functional forms, either force may dominate.
benefit cuts on asset drawdowns, spousal labor supply, and similar outcomes. Second, although I observe realized short- and long-term benefit claims (including net benefit levels) prior to 2005, data on long-term benefit receipt are frequently missing in 2005 and 2006 as a result of administrative transitions during the rollout of Hartz IV. These data gaps prevent me from analyzing the realized durations of long-term benefit receipt, but they pose no other challenges to my analysis. Other limitations include minimal detail about hours worked, topcoding of the earnings variable at the contribution limit for social security, and a lack of direct information about UI eligibility for workers who never initiate a UI claim. Finally, the underlying social security data exclude civil servants and the self-employed. The source data cover about 80 percent of total German employment.

4.2 Constructing the estimation sample

My core estimation sample consists of prime-age displaced workers who entered UI during 2001–2005. Restricting attention to UI claimants excludes both individuals who are ineligible for UI and those who, though eligible, choose not to take up benefits. This restriction is needed for the accurate calculation of potential short-term benefit duration, the key source of identifying variation used in this paper.

Using the AED, I first select UI claims initiated between January 1, 2001 and December 31, 2005. To ensure that I am capturing new unemployment spells, rather than the resumption of benefits after brief interruptions, I drop claimants who received short-term benefits in the 90 days preceding the new claim. Second, I restrict to claimants aged 25–54 at entry into UI to abstract from apprenticeship training, higher education, and retirement decisions. Third, I restrict to claimants who separated from a job covered by social insurance sometime in the 30 days preceding UI receipt. About three-quarters of new UI spells satisfy this criterion, with the remainder preceded either by unrecorded statuses like self-employment, civil service, and university enrollment or by voluntary quits, which preclude a worker from claiming benefits for 12 weeks after separation. Requiring an observed separation allows me to better measure the start of nonemployment, and it ensures that I always observe the features of the worker’s previous job. Since quitters are excluded, the sample consists of displaced workers.

Given these restrictions, some people appear in multiple (disjoint) UI spells. I retain all such spells, so that my estimates are representative of new flows into UI. I cluster standard errors by individual throughout the paper, and I show later that my results are robust to randomly selecting one UI claim per individual.

Unemployment, employment, and benefit receipt are recorded at daily frequency. Since benefit eligibility accrues in 60-day increments (not calendar months), I divide each UI spell into 30-day periods that I call “months”. I follow each spell until the exact date of reemployment. In Sections 5 and 6, I define reemployment as returning to a

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17 These endpoints are dictated by the AED sampling frame—which is representative of UI inflows only from 2001 onward—and by subsequent reforms that applied to UI claims initiated after February 1, 2006 (see Footnote 13). My sample includes workers who entered UI (i) before Hartz IV was enacted, (ii) between enactment and implementation, and (iii) in the year after Hartz IV took effect.

18 The specific age cutoffs that I choose correspond to special rules governing benefit sanctions for claimants under 25 (van den Berg et al., 2014) and to provisions for partial retirement that kick in at age 55 (Berg et al., 2015).

19 Individuals who transition to UI from an unobserved state are also presumably at the greatest risk of transitioning back into such a state. For instance, self-employed workers may be more likely than salaried workers to return to self-employment after exiting UI. Consistent with this reasoning, excluding untraceable spells increases the share of workers for whom I observe post-UI jobs.
job covered by social insurance, which I call a “regular job”. This employment concept excludes tax-favored “mini-jobs”, which are legally constrained to pay at most €325–400 per month and which are often held concurrently with benefit receipt (Tazhitdinova, 2016). To the extent that workers seek mini-jobs to supplement, rather than supplant, their UI benefits, transitions into regular jobs are likely to be a better measure of how long it takes workers to find gainful employment (and of their earnings potential upon doing so). I revisit mini-jobs in Section 7, where I distinguish transitions into regular jobs vs. mini-jobs. I censor unfinished spells at 24 months. Using a two-year horizon ensures that all spells are censored prior to the 2008 financial crisis, which may have had important effects on claimant behavior. I also report specifications that instead censor spells at 12 or 36 months.

Employers are required by law to report each worker’s average daily earnings at least once per year. I deflate earnings to 2005 EUR and multiply by 30 to obtain monthly earnings, which I call “wages”. I record prior wages using the final wage report for the last regular job preceding UI receipt; likewise, I record reemployment wages using the earliest wage report for the first post-UI job. To minimize the influence of outliers, I winsorize all wage measures at the 0.5th and 99.5th percentile of pre-UI wages within the estimation sample. Except where noted, I control for underlying earnings potential by assigning workers to deciles of prior wage within cells defined by sex × West/East German residence × year of UI entry.

I assign each worker to one of three education groups using an algorithm from Fitzenberger et al. (2006) to impute missing levels of education, which employers often fail to report. I code each worker as a German native or non-native based on the earliest reported nationality. I partition claimants into seven age bins (25–29, 30–34, 35–39, 40–44, 45–46, 47–51, and 52–54, where the over-45 age bins mirror the age-specific ceilings in the potential short-term benefit schedule) and three household types (unmarried, married without children, married with children) based on their age and household structure at the beginning of the claim. To control for heterogeneity in labor force attachment, I compute days of regular employment during the seven years preceding UI receipt and assign workers to one-year work-history bins, which I call “experience”.

Table 1 presents summary statistics both for the core estimation sample and for a comparison group of prime-age employed workers drawn from the representative 2 percent SIAB extract. The estimation sample comprises roughly 210,000 new UI claims among 144,000 distinct men and 127,000 new claims among 101,000 distinct women. Relative to the typical employed worker, claimants are adversely selected along a number of margins. About one-third of claims originate in the economically distressed states of the former East Germany (inclusive of Berlin), which represent just one-fifth of German employment. Mean pre-tax monthly earnings prior

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20 I drop the few claimants who hold regular jobs when entering UI, since “jobless duration” is ill-defined in these cases. Furthermore, since earnings in regular jobs typically exceed the earnings disregard for UI recipients, these cases may reflect erroneous recording of employment or benefit receipt dates. By contrast, it is not unusual for new UI claimants to hold mini-jobs on the day they enter UI. Since holding a mini-job need not preclude UI receipt, I retain such claimants when analyzing transitions into regular employment.

21 I do not censor spells for any other reason (such as withdrawal from the labor force) because (i) in most cases such people are still at risk of reappearing in the employment records later on; (ii) deregistration from unemployment may be endogenous to future UI benefits; and (iii) the 2005 UI reform created a seam in the unemployment rolls by obligating welfare recipients to register as unemployed for the first time. This data seam does not affect the employment and short-term UI records used in my analysis, but it would confound measurement of labor force exit.

22 Using unadjusted wage reports yields very similar, but slightly noisier estimates.
to job loss were €2,051 for men and €1,546 for women, considerably lower than mean earnings in the comparison group (€2,904 and €2,030, respectively), and claimants are less likely to have been employed for at least four of the preceding seven years. Claimants are younger than their employed counterparts, less likely to be German natives, and more likely to belong to the lowest education group (though female claimants are also more likely to be university educated).\footnote{Half the sample is unmarried (with or without children), and about half of married claimants have at least one dependent child. Household structure is reported in the unemployment register and hence is generally not observed for the comparison group.}

The mean initial UI benefit, equal to 60 or 67 percent of prior post-tax earnings, was €898 per month among men and €656 per month among women. Nearly a quarter of men and one-third of women exhaust their short-term benefits. After UI receipt, the distribution of completed jobless durations is markedly bimodal: among men, 52.3 percent of spells end within the first six months, but fully 22.4 percent last over two years; reemployment rates among women are uniformly lower but similarly bimodal. Among claimants returning to regular work within two years of UI onset, average monthly earnings in the first new job were €1,936 for men and €1,465 for women, about 5 percent lower than average pre-UI earnings in the full sample.

The research design presented below hinges on accurate measurement of potential short-term benefit duration. Although the IAB data do not explicitly record potential duration at the start of a new UI claim, I can infer it from realized UI duration together with a variable recording unused benefits, if any, remaining at the end of a UI spell. \footnote{Let $R$ denote the duration of these residual benefits, let $D$ denote the completed duration of a short-term benefit spell, and let $P(a)$ denote the age-specific maximum benefit duration, with all variables now expressed in days. I compute start-of-spell potential benefit duration as $P \equiv \min(P(a), D + R)$. That is, I set the benefit duration equal to the (observed) completed duration plus any time remaining in the worker’s claim, overriding the result if it exceeds the legal maximum. This procedure yields sensible results; for instance, the distribution of $D + R$ has large point masses at the age-specific maximums $P(a)$, as expected given the eligibility rules. Because I observe year but not date of birth, I cannot perfectly determine $P(a)$ for workers who turn 45, 47, or 52 in the year of initial UI receipt. In these ambiguous cases, I make the conservative assumption that a claimant’s birthday occurs before the beginning of the UI claim. This assumption minimizes the number of benefit durations that I override.}

Figure 4 plots the distribution of potential short-term benefit durations across new UI spells. About half of all claimants are located at their age-specific duration ceilings, especially the 12-month ceiling that applies to workers under age 45. Just under one-fifth of claimants (necessarily ages 45–54) have potential durations exceeding 12 months. A similar fraction have durations less than or equal to six months. I exploit variation in benefit eligibility between and within age groups to identify the causal effects of Hartz IV.

### 4.3 Hazard specification

In this section, I develop a dynamic econometric model that separately identifies the pre-reform “main effect” of benefit exhaustion and the incremental effect of the benefit changes induced by Hartz IV. The model allows me to control flexibly for calendar time effects, compositional changes, and other important determinants of individual employment prospects. Later, I will extend the model to look at wages and job types in conjunction with durations.

Let worker $i$ begin a UI spell on date $u_i$. I group the data into 30-day increments, indexed by $d$.\footnote{A disproportionate share of jobs begin on the first of the month, giving rise to an inherent periodicity that makes 30-day intervals a natural choice. Using 15-day periods yields similar results.} For each worker, I define two key durations corresponding to changes in the UI benefit level (superscripted with $E$ for exhaustion or $H$ for Hartz). The first of these, $d^{PE}_{i}$, is the earliest duration at which the worker has exhausted
short-term benefits. Formally,

\[ d^E_i \equiv \min\{d \in \mathbb{N} \mid 30d \geq P_i\}, \quad (4.1) \]

where \( P_i \) denotes potential days of short-term benefits at the outset of the spell. The second event, \( d^H_i \), denotes the earliest duration at which the worker receives long-term benefits under the post-reform rules. Let \( d^{2005}_i \equiv \min\{d \in \mathbb{N} \mid u_i + 30d \geq \text{January 1, 2005}\} \) denote the first duration observed after the reform legislation takes effect. Then

\[ d^H_i \equiv \max\{d^E_i, d^{2005}_i\}, \quad (4.2) \]

i.e., the larger of short-term exhaustion and the Hartz IV implementation date. Hence \( d^H_i \), which may or may not coincide with \( d^E_i \), is the duration at which Hartz IV “bites” for a given worker. Figure 6 plots hypothetical examples of these events for successive cohorts of claimants with 12 months of potential benefits at baseline.

Following common practice in the UI literature, I estimate a discrete-time proportional hazard model using the complementary log-log link (e.g., Prentice and Gloeckler 1978; Meyer 1990). \(^{26}\) Letting \( D_i \) denote the completed jobless duration, I specify the conditional probability of being reemployed during the 30-day interval \((d - 1, d] \) as

\[ \lambda_{id} \equiv \Pr(D_i = d \mid D_i > d - 1) = 1 - \exp(-\exp(x_{id}^\prime \beta)), \quad (4.3) \]

where the instantaneous log hazard rate is given by

\[ x_{id}^\prime \beta = \alpha_d + \gamma_t + z_{id}^\prime \phi + \sum_{k=-9}^{4} \delta^E_k 1\{\tau^E_{id} = k\} + \sum_{k=-9}^{4} \delta^H_k 1\{\tau^H_{id} = k\} \quad (4.4) \]

and \( t \equiv u_i + 30d \) denotes the calendar date at the end of the interval. I estimate the model via maximum likelihood, with standard errors clustered by individual to allow for arbitrary correlation for workers with repeated spells.

In Equation 4.4, \( \alpha_d \) represents a full set of duration dummies, allowing job-finding rates to vary freely as a function of months since beginning a claim—in the parlance of survival analysis, I allow for a nonparametric baseline hazard. The shifters \( \gamma_t \) control for a variety of effects linked to calendar time. First, I include a full set of quarter \( \times \) year interactions, allowing the hazard rate to evolve flexibly in response to changes in labor market conditions and other aggregate time effects. Second, I include a full set of month dummies (not interacted with year) to absorb higher-frequency calendar effects, due for example to hiring associated with the fiscal year. Third, with slight abuse of notation, I also take \( \gamma_t \) to include interactions between quarter dummies and a set of 3-month duration bins, constructed by partitioning durations into the segments \{1–3, 4–6, \ldots , 22–24\}. These interactions, which live at the \( d \times t \) level, allow for seasonal fluctuations that differentially affect workers early vs. late in their spells. \(^{27}\) \( z_{id} \) is a set of additional control variables, specified below.

\(^{26}\) The complementary log-log formulation can be derived directly from the survival function under the assumption that the instantaneous hazard rate is constant within 30-day intervals. I estimate all hazard models in Stata using the command \texttt{cloglog}.

\(^{27}\) Recalls are a simple example: they are concentrated in the spring, and they typically occur within six months of layoff. Including these seasonal controls yields smoother estimates, but my results are qualitatively and quantitatively robust to omitting them.
The key explanatory variables are flexible functions of event time relative to each benefit cut, defined as

\[ \tau_{E}^{id} \equiv \min\{d - d_{E}^{id}, 4\} \quad \text{(months relative to short-term benefit exhaustion)} \]

\[ \tau_{H}^{id} \equiv \min\{d - d_{H}^{id}, 4\} \quad \text{(months relative to reform-induced benefit cut)} \]

(4.5)

The associated coefficients \( \delta_{E}^{k} \) and \( \delta_{H}^{k} \) allow the hazard rate to vary flexibly in a window around each benefit change. Note that each omitted group comprises periods 10 or more months before the benefit change, and that I pool periods 4 or more months after the change into a single coefficient. I report the normalized hazard ratios exp(\( \hat{\delta}_{E}^{k} \)) - 1 and exp(\( \hat{\delta}_{H}^{k} \)) - 1, which represent the predicted proportional change in the instantaneous reemployment hazard associated with event time \( \tau_{E}^{id} \) or \( \tau_{H}^{id} \) with respect to the corresponding benefit change, relative to the predicted hazard 10 or more months before this event occurs. In using observations for which benefit changes lie far in the future as the reference group, I am implicitly assuming that the causal effects of benefit cuts vanish at long durations. This modeling choice is justified by the theoretical result in Section 3 that behavioral responses to future benefit cuts tend to zero at sufficiently long horizons. To the extent that claimants already start responding to future benefit cuts as early as 10 months beforehand, my estimates will be a lower bound on the true causal effect.

Without additional controls, these event-time variables would be mechanically correlated with age and experience, the determinants of potential benefit duration. In all specifications, therefore, \( z_{id} \) includes controls for seven age bins and for one-year bins of time worked in the seven years preceding UI receipt. I allow the shape of the hazard function to vary flexibly with age and experience by interacting each of these controls with 3-month duration bins. The remaining control variables in \( z_{id} \) account for other demographic characteristics that are correlated with job-finding rates. Given the sharp West/East disparity in economic conditions, I control for East German residence interacted with 3-month duration bins. Finally, I include dummies for deciles of prior wage, three education groups, German nationality, and three household types. I measure all demographic characteristics at the onset of UI receipt, so that they are fixed within each spell.

I conduct the analysis separately for men and women. Jobless durations differ markedly by sex, and disparities in average spousal earnings provide an a priori reason why the effects of Hartz IV might differ by sex.

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28 The chosen endpoints are to some degree arbitrary. Note, however, that if the “left” endpoint were to exceed 12 months pre-exhaustion, then some of the coefficients would be identified solely by claimants with over 12 months of benefits (who comprise under one-fifth of the sample). Similarly, if the “right” endpoint were to extend far beyond 4 months, then some of the post-Hartz coefficients would be identified by only a small subset of cohorts and benefit durations. My choices trade off flexibility against these considerations.

29 In the terminology of Abbring and van den Berg (2003), this supposition embodies the “no anticipation” assumption that underlies identification in timing-based research designs.

30 For example, a worker who is entitled to only 6 months of benefits by virtue of limited experience must have \( \tau_{E}^{id} \geq -6 \). Similarly, older workers are less likely to be observed with high values of \( \tau_{E}^{id} \) and \( \tau_{H}^{id} \), since their short-term benefits typically last longer.
5 Effects on Jobless Durations

5.1 Descriptive evidence

Before proceeding to the main hazard specification, I begin with a descriptive comparison of job-finding rates among claimants entering UI in 2001 ("pre-reform") vs. those entering UI in 2005 ("post-reform"). The 2001 cohort is de facto not exposed to Hartz IV, since all of these claims conclude (either through reemployment or through censoring at 24 months) prior to passage of the reform legislation. The 2005 cohort is fully exposed: upon exhausting short-term benefits, these claimants are immediately subject to the new long-term benefit level.

As a starting point, Figure 7 plots the raw empirical hazard functions for claimants under 45 with exactly 12 months of potential short-term benefits (the maximum possible in this age range). The hazard functions are typical of those found in studies of unemployment: job-finding rates initially rise with duration (reflecting recalls from temporary layoffs, especially among men) and subsequently decline, with an uptick in the vicinity of short-term benefit exhaustion, when claimants face benefit step-downs under both pre- and post-reform rules. At short durations, claimants belonging to the pre-reform cohort have a higher reemployment hazard, perhaps reflecting the tighter labor market of 2001. As short-term benefit exhaustion approaches, however, the post-reform hazard rate overtakes the pre-reform hazard. This simple comparison offers prima facie evidence that post-reform claimants are more responsive to short-term benefit exhaustion under the (less generous) post-reform benefit schedule.

Without variation in potential benefit duration, one cannot distinguish time since UI entry from time until exhaustion. To show that Figure 7 reflects greater responsiveness to exhaustion per se—rather than changes in the baseline hazard rate irrespective of benefit duration—I next expand the sample to include all workers who entered UI in either 2001 or 2005, regardless of their potential benefit duration. Because all of these claimants are subject to a fixed benefit schedule during the sample window, I estimate a simplified version of my hazard specification that is suitable for claimants facing only a single benefit step-down. Concretely, for each cohort \( Y \in \{2001, 2005\} \), I run a discrete-time hazard model that replaces the instantaneous log hazard rate from Equation 4.4 with

\[
\begin{align*}
\lambda'_{id} = \alpha_d + \gamma_t + \phi'z_{id} + \sum_{k=-9}^{4} \delta^Y_k \mathbf{1}\{\tau_{id}^k = k\},
\end{align*}
\]

where the event-time coefficients \( \delta^Y_k \) capture changes in job-finding as workers approach benefit exhaustion under the old and new UI rules, respectively. This exercise is in the spirit of difference-in-differences, with variation both across cohorts and, within each cohort, across claimants with different exhaustion points.

Figure 8 plots the normalized hazard ratios \( \exp(\delta^Y_k) - 1 \) obtained by estimating this model separately on the 2001 and 2005 cohorts. The blue and green series replicate a classic finding from research on UI: prior to reform, there is a clear “spike” in the reemployment hazard at the point of benefit exhaustion (Moffitt, 1985; Meyer, 1990; Katz and Meyer, 1990a).\(^{31}\) More striking is the pre/post change: the orange and red series reveal a much stronger

\(^{31}\) Card et al. (2007b) question the conventional wisdom about exhaustion spikes. Using Austrian administrative data, they find
spike at exhaustion for claimants subject to Hartz IV.

5.2 Benchmark estimates

By design, Figures 7 and 8 abstract from a key feature of Hartz IV: because no one was grandfathered under the pre-2005 rules, many claimants who entered UI under the old rules were partially exposed to Hartz IV, either directly (if they remained unemployed as of 2005) or indirectly (through forward-looking changes in search intensity or reservation wages). This applies especially to claimants who entered UI during 2002–2004, between the “pre” and “post” cohorts analyzed above. The lack of grandfathering presents a difficult identification challenge that has not been addressed by prior studies: a “clean” comparison between cohorts that are either fully exposed or not exposed to the new benefit schedule obligates the econometrician to compare cohorts spaced several years apart, even if (as in Figure 8) one can isolate within-cohort variation in exposure to benefit cuts. The long gap between pre- and post-periods amplifies the potential for intervening changes in labor demand, credit supply, or institutional reforms—such as other components of the Hartz package that were adopted in 2003 and 2004—to confound identification of the causal effects of benefit cuts. I now incorporate these interim cohorts into the analysis, enabling me to control flexibly for secular changes in job-finding unrelated to the timing of benefit cuts. Including the interim cohorts also enables me to retain a key population affected by Hartz IV: incumbent long-term claimants who were “caught in the storm” when the reform was announced.

The benchmark specification laid out in Section 4.3 allows each worker to respond to two distinct benefit step-downs: the main effect of short-term benefit exhaustion, plus the incremental effect of reform-induced benefit cuts. Figure 9 plots the estimated effect of these benefit step-downs on the hazard rate of reemployment, separately for men and women. Focusing first on men, the blue series in Figure 9a shows that the hazard rate of reemployment first declines slightly in the months leading up to short-term exhaustion, then rises sharply. Given the nonparametric baseline hazard, these estimates are identified by individual variation in potential short-term benefit duration: holding constant time since UI onset, workers differ in time to benefit exhaustion. The point estimate for $\tau^E_{id} = 0$ indicates that the hazard rate at exhaustion is about 25 percent higher than the hazard rate 10 or more months prior to exhaustion. These estimates mirror the pre-reform exhaustion effects previously shown in Figure 8.

The orange series shows the estimated causal effect of Hartz IV on the reemployment hazard. As workers approach reform-induced benefit cuts, the normalized hazard ratio rises steadily, peaking at 0.48 in the month that straddles the benefit step-down ($\tau^H_{id} = 0$). This pattern of positive hazard effects in the months preceding a benefit cut is indicative of forward-looking behavior on the part of UI claimants, as predicted by the search model in Section 3. The behavioral response is large: relative to claimants for whom Hartz IV lies 10 or more months in the future, there is a large spike in exits from registered unemployment but only a small increase in the reemployment hazard at the time of benefit exhaustion. The estimates plotted in Figures 7 and 8—as with similar plots throughout the paper—reflect true job-finding, not simply an artifact of deregistration from unemployment.

32 If incumbent claimants had been shielded from the reform, one could use a regression discontinuity design to compare claimants who enter UI just before/after January 1, 2005. These groups would face sharp differences in long-term UI generosity but similar labor market conditions and institutions. Absent grandfathering, however, these groups are equally exposed to the new benefit schedule.
future, claimants experiencing benefit cuts are 48 percent more likely to return to regular employment over a short time interval. The coefficients decline in the wake of the Hartz IV benefit change, then stabilize close to zero. Given the proportional hazards structure, the Hartz IV effects greatly magnify the pre-existing spike at benefit exhaustion. For claimants fully exposed to the post-reform benefit schedule, the hazard rate at exhaustion is estimated to be 85 percent greater than the hazard rate when exhaustion lies 10 or more months away \(= \exp(\hat{\delta}_E + \hat{\delta}_H) \cdot 1\), conditional on duration since UI entry and other observables. This is in the same ballpark as the exhaustion spike measured for post-reform male claimants in the simpler specification used in Figure 8.

Figure 9b replicates these plots for women. The results are broadly similar to those for men, with the incremental hazard effect peaking at 0.54 in the month that straddles the reform-induced benefit change. A stark difference, however, is the much stronger main effect of short-term benefit exhaustion (green series) for women than for men. This likely reflects the role of spousal income in means testing: on average, married women have higher earning spouses than married men do, and many married women are ineligible for long-term benefits under both the pre- and post-reform regimes. The pronounced exhaustion spike appears to be driven by especially sharp benefit reductions for women at this juncture. As with men, the post-reform exhaustion spike depends on the exponentiated sum of the main and incremental effects: under the new schedule, my estimates imply that the hazard rate at exhaustion is 181 percent greater than the hazard rate when exhaustion occurs far in the future, again conditioning on observables.

A question that arises here is why the Hartz IV coefficients (and those pertaining to benefit exhaustion itself) decline post-cut rather than remaining constant at a high level. In the simple search model of Section 3, where the environment is stationary after benefits run out, the hazard rate of exit rises in the run-up to a benefit cut and then stays constant. Even if this model perfectly describes individual behavior, however, unobserved individual heterogeneity can readily generate attenuation over time. Intuitively, if the workers who are most sensitive to benefit reductions return to work in advance of exhausting benefits, the claimants who remain will be the ones least responsive to benefit cuts. Such heterogeneity could stem from differences either in individual preferences (e.g., the cost of job search) or in the change in benefit generosity experienced under Hartz IV. In either case, my estimates reflect the local average treatment effect within a dynamically changing risk set. Dynamic selection is inherent to duration models (Kiefer, 1988). Perhaps for this reason, studies of benefit exhaustion generally show a falling-off of the hazard effect: labor economists speak of an exhaustion “spike”, not an exhaustion “plateau”.34

33 Consistent with this hypothesis, the spike at short-term exhaustion is much more pronounced for married women than for singles. Male claimants, by contrast, show fairly similar main effects of short-term exhaustion regardless of marital status—a plausible result given Germany’s relatively low rate of female labor force participation.

34 The UI literature has advanced alternative explanations for the exhaustion spike. Boone and van Ours (2012) argue that many job offers are “storable”, so that jobseekers strategically time their start dates to coincide with benefit exhaustion. DellaVigna et al. (2016) posit that jobseekers have reference-dependent preferences anchored to recent income. In this view, the hazard rate declines post-exhaustion because workers become accustomed to lower income and don’t search as hard.
5.3 Effect sizes

How do these hazard effects translate into effects on jobless durations? Though proportional hazard effects are informative about behavioral responses among still-unemployed workers, the overall impact depends on the underlying distribution of durations. To see this, write the cumulative reemployment rate recursively as

$$F(d) = \sum_{k=1}^{d} S(k-1) \lambda(k),$$

where $$S(\cdot) = 1 - F(\cdot)$$ is the survival function. Letting $$S^{cf}(\cdot)$$ and $$\lambda^{cf}(\cdot)$$ denote counterfactual survival and hazard rates absent reform, a useful approximation to the change in reemployment is

$$dF(d) \approx \sum_{k=1}^{d} S^{cf}(k-1) \lambda^{cf}(k) d \log(\lambda(k)).$$

(5.2)

This expression makes it clear that Hartz IV’s effects depend not only on the proportional hazard effects, but also on how many workers remain at risk and on the counterfactual hazard that the proportional effect magnifies.

Long-term benefit generosity matters more when workers tend to reach long-term unemployment and when, upon doing so, they are on the margin of finding work.

To quantify shifts in the path of cumulative job-finding, I predict successive UI cohorts’ reemployment rates both under the fitted model—which incorporates the Hartz IV benefit cuts—and under a counterfactual scenario in which these cuts do not occur. I explain the procedure in detail, as similar exercises appear later in the paper.

For each UI spell, I predict the discrete-time hazard rate at each duration as

$$\hat{\lambda}_{id} \equiv \Pr(D_i = d \mid D_i > d-1) \equiv 1 - \exp(-\exp(x_{id}’ \hat{\beta})).$$

(5.3)

Chaining these hazard rates yields each claimant’s probability of being reemployed by a given duration:

$$\hat{F}_{id} \equiv \Pr(D_i \leq d) \equiv 1 - \prod_{s=1}^{d} (1 - \hat{\lambda}_{is}).$$

(5.4)

To predict reemployment rates with the reform-induced benefit cuts shut off, I recompute this expression with the time-to-Hartz IV variable $$\tau^{H}_{id}$$ recoded to the omitted category at all durations. That is, I replace $$x_{id}’ \hat{\beta}$$ with

$$x_{id}’ \hat{\beta}^{cf} \equiv \hat{\alpha}_d + \hat{\gamma}_t + z_{id}’ \hat{\theta} + \sum_{k=-9}^{4} \delta_k^{E} \mathbb{1}\{\tau_{id}^{E} = k\},$$

(5.5)

which partials out the estimated effect of long-term benefit cuts induced by Hartz IV. Using this expression, I compute counterfactual reemployment rates $$\hat{F}_{id}^{cf}$$ using the same procedure as above. Finally, I compute the average (partial equilibrium) effect of the reform on claimants entering UI in year $$y$$ as

$$\Delta \hat{F}^{y}_d \equiv \frac{1}{N_y} \sum_{i|u_i \in y} (\hat{F}_{id} - \hat{F}_{id}^{cf}).$$

(5.6)

Table 2 reports $$\Delta \hat{F}^{y}_d$$ by entry cohort for $$d \in \{6, 12, 18, 24\}$$ months, separately for men and women. For
claimants who enter UI in 2001, the predicted effect of Hartz IV is mechanically zero: two years into a UI spell, all workers in this cohort are still at least 10 months away from experiencing benefit cuts under Hartz IV, so that  \( \hat{F}^{cf}_{id} = \hat{F}_{id} \) by construction. At the opposite extreme, claimants who enter UI in 2005 are fully exposed to Hartz IV: they encounter the post-reform regime as soon as they exhaust short-term benefits. Workers belonging to interim cohorts are affected to a greater or lesser degree depending on the timing of UI entry coupled with their potential short-term benefit duration. As expected, the effects of Hartz IV cumulate steadily for successive cohorts.

The 2005 cohort offers my best estimate of the long-run, steady-state impact of Hartz IV on jobless durations. For this cohort, Figure 10 plots the full path of estimated reemployment effects for 24 months following entry into UI. The effects accrue rapidly for the first 12 months, then decline slightly thereafter as the counterfactual series partly catch up to the factual series. The employment gains are largely persistent at 24 months, however, suggesting that benefit cuts have enduring effects on cumulative job-finding even at lengthy durations.

For statistical purposes, Germany defines long-term unemployment—not to be confused with long-term UI, whose timing varies across individuals—as a jobless spell lasting over one year. I estimate that Hartz IV increased the probability of being reemployed within 12 months of beginning a claim by 4.0 percentage points for men (relative to a counterfactual probability of 68.8 percent) and by 5.9 points for women (relative to a counterfactual probability of 51.0 percent). In proportional terms, these estimates imply that Hartz IV reduced the likelihood of entering long-term unemployment by 12.8 percent for men and by 12.0 percent for women (or by 12.4 percent overall, pooling men and women). In the spirit of Chernozhukov et al. (2013), I construct confidence intervals by drawing 500 parameter vectors using the estimated, asymptotically normal variance-covariance matrix, replicating the quantification exercise, and taking the standard deviation across estimated effects. The net employment effects are precisely estimated and easily differ from zero at the 5 percent level.

My core analyses censor incomplete spells at 24 months. To verify that my results are not sensitive to the censoring horizon, and to see whether Hartz IV’s pro-employment effects persist at even longer durations, I reestimate my benchmark specification with incomplete spells censored at either 12 months or 36 months. Table 3 reports net employment effects for the fully exposed 2005 cohort under these alternative horizons. The bottom-line magnitudes are very similar (as are the hazard effects, which I omit to conserve space). The factual-counterfactual employment gap continues to contract beyond 24 months but remains sizable even 36 months after UI entry.

5.4 Robustness

The results from the benchmark specification are robust to a variety of control strategies and sample modifications. I present these robustness checks graphically in Figure 11. Results from the same specifications (including standard errors) are also presented in Appendix Table 1. All specifications are precisely estimated, with highly significant increases in the reemployment hazard in the months preceding the change in benefit.

Specification 1 (in dark blue) replicates the benchmark Hartz IV effects from Figure 9. Specification 2
(orange) allows for compositional changes in the pool of new UI claimants by adding fixed effects for each quarter \( \times \) year of entry into UI. Although I already control for a rich set of observable covariates, the effects of Hartz IV could potentially be confounded by unobserved changes in claimant characteristics across successive cohorts.\(^{35}\) Reassuringly, this specification yields similar (and in fact somewhat larger) effects. The scope for composition bias appears to be limited, given that there is little observable time-series variation in claimant characteristics.\(^{36}\)

The next two series stress-test the logic underlying my identification strategy. First, in specification 3 (green), I allow the age \( \times \) duration and experience \( \times \) duration interactions already included in \( z_{id} \) to differ before and after July 1, 2004, when Hartz IV became salient. Suppose that, for reasons unrelated to benefit cuts, younger workers experienced a differential improvement in their job prospects around this time. Because young workers tend to have briefer potential short-term UI durations (due to the 12-month cap for workers under 45), such an improvement might spuriously lead me to overstate the pro-employment effects of exposure to benefit cuts. Adding three-way interactions between age bins, duration bins, and a post-reform dummy addresses this concern.\(^{37}\) The three-way experience interactions serve a similar function. Second, on top of these three-way interactions, specification 4 (red) allows the shape of the baseline hazard to change over time. In place of quarter \( \times \) year dummies, which allow the baseline hazard function to shift up or down proportionally over time, I interact each 3-month duration bin with the full set of quarter \( \times \) year dummies. This more flexible specification allows the reemployment hazards at short, medium, and long durations to vary independently of one another from quarter to quarter. In effect, this demanding specification compares the reemployment hazards of individuals who are observed at similar durations at a given point in time, but who differ in the timing of benefit changes. In both of these specifications, the results are strikingly stable, with the point estimates increasing modestly in magnitude.

Some individuals in my estimation sample appear in multiple, disjoint spells. Although it is not obvious that repeat spells present any problems—especially since I cluster on individual throughout—specification 5 (light blue) restricts to a single UI spell per individual by selecting one spell at random among people who experience multiple spells. Again, this specification yields very similar estimates.\(^{38}\)

The final series, specification 6 (brown), addresses a more specific selection concern. Under a standard model of UI take-up (Anderson and Meyer, 1997), reductions in the benefit level should deter some individuals

\(^{35}\) For example, workers laid off during 2004–2005—when unemployment was at its peak—are likely to be positively selected on unobservables (e.g., work ethic) relative to workers laid off during the tighter labor market of 2001–2002 (Nakamura, 2008; Mueller, 2015). Since these late-sample workers are more exposed to Hartz IV, failing to control for unmeasured cohort effects could bias me towards overstating the pro-employment effects of the reform.

\(^{36}\) Appendix Figure 2 plots the mean predicted jobless duration by quarter of entry into UI, based on a Weibull model of reemployment hazards estimated on UI claims that begin in 2001. Men exhibit no trend in predicted duration, apart from seasonal fluctuations that likely reflect the greater incidence of temporary layoffs in the first and fourth quarters of each year. Women exhibit a gradual decline in predicted duration, but there is no discontinuity or trend break at the time of Hartz IV.

\(^{37}\) Two-way interactions between age and the post-reform dummy would address the same concern, but interacting with duration as well is more flexible.

\(^{38}\) A related concern is that individuals with very limited benefit eligibility—either because they qualified through seasonal employment or because they are drawing down a prior, partially exhausted claim—may be driving the increased exhaustion spike. Such individuals are disproportionately represented among observations for which \( r_{id}^H = 0 \), since their benefit exhaustion occurs before many of them have had time to exit the risk set. I obtain very similar results if I restrict to claimants with at least 6 months of potential short-term benefits, the starting-point for a fresh UI entitlement.
from claiming UI. Moreover, the take-up “compliers” (who would claim benefits under the pre-Hartz rules but not the post-Hartz rules) may differ unobservably from those who would claim benefits under both scenarios.\(^{39}\) To limit the scope for such differential take-up, I restrict the sample to workers whose UI claims begin before July 2004. This restriction leads to somewhat larger estimates, especially for men. One possible explanation for this result is that, insofar as Hartz IV did deter UI take-up, the post-July 2004 job losers most sensitive to the onset of the reform may not enter my sample at all.\(^{40}\) Another is that Hartz IV may have had stronger effects on workers who were caught by surprise than on those who entered UI after the new regime was already in place. Regardless, there is no indication that differential take-up is causing me to overstate the pro-employment effects of UI reform.

These specifications exploit variation in potential benefit duration stemming from both age and experience. This fact invites an additional test: by isolating each source of variation in turn, I can gauge if they yield similar results, as my story would suggest. I explore this idea in Appendix Figure 4. The first two specifications, in blue and orange, reestimate the benchmark specification using workers ages 25–34 and 35–44, respectively. Since short-term benefits max out at 12 months for workers under 45, variation in potential duration within these subsamples is driven entirely by differences in prior employment and benefit histories. Conversely, the red series restricts attention to claimants (of any age) with maximal potential duration given their age bracket. Here, the identifying variation stems solely from differences in age. Results are qualitatively and quantitatively similar across specifications.

As a final robustness check, I estimate the benchmark specification separately for 36 cells defined by sex, region, three household types, and terciles of initial short-term benefit level.\(^{41}\) Appendix Figure 5 reports the effect of Hartz IV on each group’s job-finding hazard in the month of the Hartz IV benefit change (\(\tau_H = 0\)).\(^{42}\) Almost every cell experiences a positive effect, suggesting that the Hartz IV benefit cuts—though difficult to measure accurately given data limitations—were pervasive enough to spur reemployment for a wide range of claimants. Exposure to the new benefit schedule increases job-finding in both West and East Germany, weighing against region-specific alternative explanations, and for both single claimants and childless couples. I find more mixed results among married claimants with children, consistent with benefit simulations suggesting that many such claimants were ineligible for long-term UI under both pre- and post-reform rules.\(^{43}\)

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\(^{39}\) Although Anderson and Meyer consider a one-tier, fixed-length UI regime (as in the United States), their comparative static that benefit cuts deter UI take-up is easily extended to the case of reductions in a second benefit tier. Using the envelope theorem, it can also be shown that the utility change from a (small) cut to long-term benefits is increasing in the likelihood of reaching long-term unemployment and decreasing in the potential duration of short-term benefits. Given these differential welfare consequences, it is plausible that Hartz IV would lead to differential selection into UI.

\(^{40}\) I attempted to estimate the effect of Hartz IV on UI take-up, exploiting variation in imputed potential benefit duration. The results were too imprecise to be informative, reflecting both measurement error in the imputation of UI eligibility and the smaller 2 percent SIAB sample available for this exercise. Seasonality in job loss poses additional challenges for studying UI takeup.

\(^{41}\) Much of the complexity of changes in the benefit schedule stems from spousal income, which is included in the means test, and from the presence of children, who yield supplementary benefits. For a given household structure, prior earnings—which map one-to-one into the short-term benefit level, after taxes—are a key determinant of the reform-induced change in UI generosity.

\(^{42}\) Because sample sizes are considerably smaller when I split the sample by household type and benefit tercile, I pool the event months \{−9, −8, −7\}, \{−6, −5, −4\}, \{−3, −2, −1\}, \{0\}, \{1, 2, 3\}, and \{4, 5, 6, ...\} to improve precision. The subgroups exhibit the same hump-shaped hazard effects seen in Figure 9: for 29 of the 36 cells, the peak hazard effect occurs when \(\tau_H = 0\) (and for 5 of the remaining 6 cells, the peak effect occurs 1–3 months prior to the benefit change).

\(^{43}\) These estimates offer partial support for the idea that claimants exposed to steeper cuts should exhibit larger behavioral responses. Across these cells, the peak hazard effect is negatively correlated with the mean simulated change in potential household income induced by Hartz IV (\(\rho = −.45\) among men, \(\rho = −.37\) among women). Several factors may contribute to this fairly weak relationship. First, my simulated benefit changes may suffer from classical measurement error, which would attenuate these correlations. Second, even if...
5.5 A placebo exercise

Is there anything special about January 2005, when Hartz IV took effect? Consider two threats to identification. First, suppose that German UI claimants became steadily more responsive to short-term benefit exhaustion over the course of my sample period for some reason unrelated to the Hartz reforms. This could occur if, for example, the supply of consumer credit decreased as labor market conditions worsened in the early 2000s. Second, suppose that the earlier components of the Hartz package—implemented in January 2003 and January 2004—differentially affected reemployment rates among jobless workers who were close to exhausting benefits. Either phenomenon could conceivably result in spurious “Hartz IV” effects even if the UI reform itself had no causal effect on job finding. The reason, heuristically, is that in hazard models allowing a structural break in 2005, both of these forces could potentially load onto the post-break variables, even if the break itself is mistimed.

To assess these threats, I estimate placebo specifications in which I alter the assumed date of the UI reform. Concretely, for each year $Y \in \{2001, 2002, 2003, 2004, 2005\}$, I use the 2 percent SIAB dataset to construct a sample of new UI claims initiated between January 1 of year $Y - 4$ and June 30 of year $Y$. I then reestimate the benchmark hazard model with the true Hartz IV event-time variable $\tau_{id}^H$ replaced by $\tau_{id}^{H,Y}$, where $\tau_{id}^{H,Y}$ is computed as though Hartz IV took effect on January 1 of year $Y$. Note that $\tau_{id}^{H,Y} \equiv \tau_{id}^{H,2005}$, so that for $Y = 2005$ this model reestimates the causal effects of Hartz IV using the SIAB dataset. I censor incomplete spells on June 30 of year $Y$ to avoid misattributing the causal effects of Hartz IV itself to the placebo reforms. I pool all post-event periods into a single coefficient, since the abbreviated post-“reform” period—lasting only six months—makes it difficult to separately identify the event-time coefficients for $\tau_{id}^{H,Y} \geq 1$.

Figure 12 shows that 2005 was indeed different. The blue series in each panel—corresponding to the true 2005 reform—closely parallels my benchmark estimates, with the hazard effect peaking at about 60 percent for men and 45 percent for women. For pseudo-reform years 2001, 2002, and 2003, the estimated placebo effects are close to zero, providing no evidence of a longstanding trend towards greater sensitivity to short-term exhaustion. The stability of the placebo estimates is especially encouraging because Germany’s labor market picture was changing rapidly in these years, with steep increases in unemployment throughout the early 2000s. For 2004, I do find positive (and significant) placebo effects, though they are reassuringly smaller than the Hartz IV effects themselves in the months leading up to benefit cuts.

In Appendix B, I consider three possible explanations for the modest 2004 placebo effects: anticipatory reactions to Hartz IV prior to July 2004, delayed responses to an earlier tightening of the asset means-test for long-term benefit cuts under the new rules, they may not have known precisely how idiosyncratically steep their own cuts would be, again attenuating these correlations. Third, changes in benefit cuts may be correlated with responsiveness to a given cut. For example, high-earners tended to face steeper cuts under Hartz IV, but they may be less responsive to marginal changes in long-term benefits because they are at lower risk of experiencing very long jobless spells.

Recall that the AED is representative only of spells that begin in 2001 or later. For that reason, the SIAB is better suited to this exercise. Slightly different age cutoffs for the maximum potential benefit duration applied to claimants who entered UI before April 1999 (some of whom appear in the 2001–2003 placebo specifications). When computing potential benefit duration, I use whichever cutoff was in effect at the onset of the claim. Placebo results are robust to excluding pre-April 1999 entries into UI.
term benefits, and changes in the frequency of UI benefit sanctions. As explained in the appendix, allowing the baseline hazard to evolve flexibly over time—which more stringently partials out changes in duration dependence unrelated to benefit step-downs—strengthens the 2005 Hartz IV effects while attenuating the 2004 placebo effects (Appendix Figure 3), lending further support to a causal interpretation of my estimates.

5.6 Timing of responses among incumbent long-term claimants

I have shown that reemployment hazards rise as UI claimants approach benefit changes induced by Hartz IV. To provide further evidence of the temporal link between the implementation of Hartz IV and increased job-finding among reform-exposed claimants, I now examine job-finding rates among incumbent long-term claimants, all of whom were subject to immediate benefit changes if they were still unemployed on January 1, 2005. Among workers receiving long-term benefits prior to the reform, post-reform changes in relative reemployment hazards are strongly correlated with a proxy for the size of the benefit cut induced by Hartz IV.45

Because Hartz IV replaced a wage-indexed benefit with a uniform benefit unrelated to pre-UI earnings, claimants with greater prior earnings generally received steeper benefit cuts under the new rules. To explore this idea, I use the SIAB dataset to construct a representative 2 percent sample of new long-term UI claims originating between 2000 and 2004. To ensure that I observe baseline covariates, I restrict to workers who held a regular job sometime within the 3 years preceding long-term UI receipt. To proxy for the benefit change that occurs on January 1, 2005, I partition claimants into terciles based on their initial long-term benefit level (which I observe net of means testing), stratifying by sex, region, household type, and year of entry into long-term UI. Finally, I estimate a complementary log-log model of duration until reemployment. The explanatory variables are a full set of duration effects, allowing the baseline hazard rate to evolve nonparametrically with time since entry to long-term UI (stratified by West/East residence); dummies for age bins, German nationality, and three household types; calendar time effects at quarterly frequency; and interactions of these time effects with indicators for the second and third benefit terciles. Because the sample becomes thinner at long durations and at later calendar dates, I censor incomplete spells 36 months after entry to long-term UI or at the end of 2006, whichever comes first.

Figure 13 plots the time effect \( \times \) benefit tercile interactions, which show how the reemployment hazards of medium- and high-benefit claimants evolve over time relative to those of low-benefit claimants. Between 2000 and mid-2004, reemployment hazards for the second and third benefit terciles closely track those for the lowest tercile. Towards the end of 2004, however, there is a clear divergence: consistent with the hypothesis that cuts were steeper for those with generous pre-reform benefits, the second and (especially) third terciles show employment gains relative to the first.46 These interactions reveal only the relative effect of Hartz IV as a function of baseline

45 As noted in Footnote 43, such comparisons may be confounded by (i) imperfect comprehension among claimants, (ii) measurement error in my benefit simulations, or (iii) treatment effect heterogeneity across groups experiencing different benefit cuts. These three potential confounds should be less severe in a sample of claimants who reach long-term UI, since (i) such claimants have greater familiarity with the UI system, (ii) I observe their pre-reform long-term benefits net of means testing, and (iii) conditioning on long-term unemployment should truncate the distribution of unobserved heterogeneity that gives rise to heterogeneous treatment effects.

46 Of the 60 time \( \times \) tercile interactions that precede 2004Q3 (30 each for men and women), only five are significantly positive at the
benefits, with the main effect difficult to separate from any background time trends.\footnote{5 percent level in a two-sided test (three are significantly negative). By contrast, more than half of the interactions are positive at the 5 percent level from 2004Q3 onward (none are significantly negative). I omit confidence intervals to avoid cluttering the figure.} It is also possible that the differential trajectories of workers in each tercile partly reflect different elasticities of job-finding to a given change in hazard, rather than being driven solely by differences in the benefit changes they encounter. Nonetheless, the stark time patterns in Figure 13 establish a tight temporal link between the post-Hartz IV shift in job-finding dynamics and concurrent changes in UI generosity.

5.7 Partial equilibrium impact on the steady-state unemployment rate

Having argued that my estimates represent the causal effects of long-term benefit cuts on individual jobless durations, I conclude this section with a brief discussion of their possible implications for the broader German labor market. Although a full reckoning of Hartz IV’s aggregate impact is beyond the scope of this paper, I use a back-of-the-envelope calculation to gauge what my partial equilibrium estimates, taken at face value, imply for the steady-state unemployment rate, and in particular for its long-term component.

Let $u_{ST}$ and $u_{LT}$ denote the short- and long-term unemployment rates, defined in Germany as the number of workers unemployed for, respectively, under/over 12 months as a share of the overall labor force. Let $D_{ST}$ be the expected number of months that a newly unemployed worker spends in short-term unemployment: hence $D_{ST} \equiv \sum_{d=1}^{12} S_d$, where $S_d$ is the probability of being unemployed for at least $d$ months. Likewise, let $D_{LT} \equiv \sum_{d=13}^{\infty}$ denote the expected number of months that such a worker will spend in long-term unemployment. It is convenient to decompose this expression into $D_{LT} \equiv D_{13-24} + D_{25-\infty}$, distinguishing the second year of unemployment from what follows. Next, let $q$ be the exogenous monthly rate of job separation, assumed to be constant throughout this exercise. Abstracting from flows in and out of the labor force, steady-state unemployment equals:  

$$u_{ST} = \frac{D_{ST}}{1/q + D_{ST} + D_{LT}}$$  

$$u_{LT} = \frac{D_{LT}}{1/q + D_{ST} + D_{LT}}$$

On the eve of Hartz IV, Germany’s overall unemployment rate was about 10 percent, divided equally between short-term and long-term unemployment. I therefore begin by setting $u_{ST} = u_{LT} = 5.0$ percent. In the absence of Hartz IV, my hazard estimates imply that $D_{ST}^{cf} = 6.79$ months and $D_{13-24}^{cf} = 3.62$ months. Given these values, I calibrate $q = .82$ percent and $D_{25-\infty}^{cf} = 3.17$ months to satisfy Equations 5.7 and 5.8, so that $D_{LT}^{cf} = 6.79$ months. With Hartz IV in place, my hazard estimates imply smaller values $D_{ST} = 6.43$ months and $D_{13-24} = 3.10$ months.

\footnote{The calendar-time coefficients are negative after Hartz IV takes effect, so that the lowest tercile exhibits lower reemployment rates after the reform than before it. The decline could represent benefit increases for this group of claimants, congestion externalities due to Hartz IV’s impact on aggregate labor supply, or other, unrelated changes in labor market conditions.}

\footnote{To see this, imagine an infinitely lived worker toggling between employment spells of average duration $1/q$ and unemployment spells of average duration $D_{ST} + D_{LT}$. Each job-layoff cycle lasts an average of $1/q + D_{ST} + D_{LT}$ months, of which $D_{ST}$ are spent in short-term unemployment and $D_{LT}$ are spent in long-term unemployment. If the job-finding rate $f$ is constant, then $D_{ST} + D_{LT} = 1/f$ so that $u \equiv u_{ST} + u_{LT} = \frac{q}{q+f}$; that is, these expressions nest the familiar steady-state formula given constant transition probabilities.}

27
months. Due to censoring, my hazard model is silent as to how Hartz IV affects job-finding after 24 months. If, conservatively, I assume that Hartz IV has no impact on the hazard rate of reemployment beyond this point, then $D_{25-\infty}$ falls to 2.68 months. The steady-state unemployment rates fall to $u_{ST} = 4.8$ percent and $u_{LT} = 4.3$ percent.

Though stylized, this back-of-the-envelope calculation suggests that Hartz IV may have reduced Germany’s unemployment rate by about 0.9 percentage points—a measurable, if not decisive, share of the dramatic 6.6 percentage point decline in unemployment that Germany experienced between December 2004 and December 2015. Strikingly, the steady-state impact of Hartz IV is driven almost entirely by a 0.7 percentage point reduction in the long-term unemployment rate. This finding underscores the point that long-term UI generosity is especially relevant for prolonged jobless spells, and it echoes the Ljungqvist and Sargent (1998, 2008) hypothesis that generous UI benefits may contribute to persistently high levels of long-term unemployment.

My research design identifies partial equilibrium impacts of benefit cuts on individual job-finding. In general equilibrium, the direct effect of increased individual search effort may be either mitigated by congestion externalities or amplified by job creation. In equilibrium search-and-matching models, these indirect effects depend on the intensity of changes in individual search effort. Credible estimates of the direct effect can therefore help researchers gauge the possible importance of the congestion and job-creation channels. In addition to their effects on the unemployed, benefit cuts may also affect employed workers by worsening their outside options in the event of job loss. Reductions in UI generosity may deter separations or put downward pressure on wages through shifts in bargaining power. Although my partial equilibrium estimates are only one part of the puzzle, I regard them as a useful input into future efforts to calibrate the macroeconomic effects of Hartz IV.

6 Effects on Reemployment Wages

I have shown that Hartz IV spurred UI claimants to find jobs faster. But how does cutting long-term UI benefits affect the wages they receive on those jobs? An oft-cited rationale for UI is that, in addition to smoothing consumption, it enables liquidity-constrained jobseekers to prolong their job searches and thereby obtain higher-paying positions. In keeping with this view, critics of Hartz IV allege that cutting UI benefits has contributed to rising German wage inequality and to falling real wages at the left end of the earnings distribution (Dustmann et al., 2009). As the model in Section 3 suggests, however, reducing long-term UI generosity has theoretically ambiguous effects on post-UI wages: benefit cuts can indeed lower wages by depressing reservation wages or weakening workers’ bargaining power, but they can also raise wages by shortening jobless spells that erode earnings capacity (Nekoei and Weber, 2016; Schmieder et al., 2016). Identification of wage effects is further complicated by a standard

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49 Seasonally adjusted unemployment rates published by Germany’s Federal Statistical Office, based on the International Labor Organization’s definition of unemployment. Soon after Hartz IV, a companion measure reduced the maximal duration of short-term benefits for older workers entering UI after February 1, 2006 (see Footnote 13). There is a complementarity between reducing long-term UI benefit levels and reducing short-term benefit durations, as the latter causes the former to apply earlier in a worker’s spell. Accounting for this interaction effect would magnify the estimated partial equilibrium impact of Hartz IV.

50 Though absent from the theoretical framework in Section 3, bargaining effects might arise either in general or in partial equilibrium, depending on whether employers can identify how UI reform affects particular applicants’ outside options. In the wage specification,
selection challenge: wages are only observed for those who eventually become reemployed (Heckman, 1979; Ham and Lalonde, 1996). This section develops an empirical framework for quantifying the net wage impact and for disentangling it into these three channels.

6.1 Accepted wages in the lead-up to benefit changes

I begin by estimating the effects of benefit cuts on the wages workers receive in their first regular job after UI, conditional on their completed jobless duration. Let \( w_{id} \) denote the log ratio of reemployment wages to pre-UI wages for a worker reemployed \( d \) months into her UI spell. By analogy with the benchmark hazard specification laid out in Section 4.3, I run OLS regressions of log wages on remaining benefit durations at the moment of hiring:

\[
    w_{id} = \tilde{\alpha}_d + \tilde{\gamma}_t + z_{id}' \tilde{\phi} + \sum_{k=-9}^{4} \tilde{\delta}^E_k 1\{\tau_{id}^E = k\} + \sum_{k=-9}^{4} \tilde{\delta}^H_k 1\{\tau_{id}^H = k\} + \varepsilon_{id}.
\] (6.1)

The explanatory variables are identical to those used in the hazard model (with coefficients distinguished by tildes). As before, the event-time coefficients \( \tilde{\delta}^E_k \) and \( \tilde{\delta}^H_k \) allow wages to evolve flexibly as workers approach and then pass the two benefit changes. The duration dummies \( \tilde{\alpha}_d \) allow wages to vary with completed unemployment duration, as a result of either structural duration dependence or dynamic selection. The time effects \( \tilde{\gamma}_t \) control both for shifts in aggregate wage dynamics and for seasonal patterns in wage offers. As before, the vector of demographics \( z_{id} \) includes indicator variables for each decile of pre-UI wages, allowing the ratio of wages before/after unemployment to vary flexibly throughout the distribution of prior wages. I again cluster by individual to allow for correlated errors between multiple UI spells experienced by the same individual.

Figure 14a and Figure 14b plot, for men and women respectively, estimated changes in log reemployment wages as workers approach the two step-downs in the benefit schedule. The lefthand panel in each figure shows the main effect of benefit exhaustion, which is active both before and after the reform. Men exhibit a gradual deterioration of reemployment wages as they approach short-term benefit exhaustion, with a sharp decline in the month of exhaustion. Women initially show no change in wages, but wages fall sharply a month prior to exhaustion. Men (women) who accept jobs in the month following benefit exhaustion, when \( \tau_{id}^E = 1 \), receive 6 percent (10 percent) lower wages than those reemployed 10 or more months prior to exhaustion, conditional on previous earnings, completed duration, and a rich set of observable characteristics. These negative effects—identified by cross-sectional variation in potential benefit durations among observationally similar claimants with the same ex post jobless duration—offer suggestive evidence that reservation wages fall sharply as workers approach benefit cuts. My finding of lower wages in the vicinity of a benefit step-down is consistent with Schmieder et al. (2016), who find that workers reemployed in the month of short-term UI exhaustion accept lower wages.\(^{51}\)

\(^{51}\) See their Figure 6, which uses a regression discontinuity at age 45 to compare the path of reemployment wages for German workers eligible for either 12 or 18 months of short-term benefits. My results differ from Schmieder et al.’s in two ways: first, I find that wage responses among men begin several months prior to exhaustion; second, for both men and women, reemployment wages remain...
The right-hand panels show the additional impact of benefit cuts induced by Hartz IV. Among claimants with more than four months remaining until the new rules kick in, wages are insensitive to future benefit cuts. Wages decline sharply thereafter, however, with effects peaking around 8 percent for both men and women taking jobs in the month after the benefit cut. The pattern of falling wages in advance of the benefit cut is indicative of anticipatory behavior on the part of jobseekers.\textsuperscript{52} Interestingly, however, changes in the reemployment hazard precede changes in reemployment wages by several months, suggesting that workers faced with long-term benefit cuts may first increase their search effort, then lower their reservation wages if they still remain unemployed. As with the hazard effects in Section 5, the total impact of benefit exhaustion under the new rules is given by the sum of the main and incremental effects: for fully exposed claimants, jobs accepted just after benefit exhaustion pay roughly 15 percent lower wages than jobs accepted 10 or more months before benefits run out.

Appendix Figure 6 explores the robustness of these estimates to control strategies analogous to those used for the earlier hazard specification. Adding additional controls—such as quarter-of-entry dummies to soak up unobserved heterogeneity across cohorts, interactions between the age/experience effects and an indicator for the post-reform period, and time-varying duration effects—attenuates the effects by up to about one-third for men and, in some specifications, by over one-half for women, but the qualitative patterns are similar.\textsuperscript{53} A balanced reading of the evidence is that Hartz IV lowered reemployment wages by about 4 to 8 percent for men accepting jobs in the immediate aftermath of benefit cuts, with qualitatively similar but noisier and perhaps smaller effects for women. These effects augment the “drop at exhaustion” already evident in wage offers accepted prior to the reform.

### 6.2 Quantifying and decomposing the net wage effect

As noted above, Figure 14 and Appendix Figure 6 give only a partial picture of the overall wage effect, since Hartz IV may also impact wages indirectly by shortening durations and altering selection into work. Much as I previously used the fitted duration model to quantify effects on jobless durations for claimants fully exposed to the new benefit schedule, I now translate these point-in-time wage impacts into average effects on the wage paid in a Hartz-exposed worker’s first post-UI job. To unpack the underlying mechanisms and to deal with observable changes in the pool of reemployed workers, I then decompose the overall impact into direct, indirect, and selection terms.

Following Schmieder et al. (2016), it is useful to distinguish changes in the path of reemployment wages from shifts along the path. Formally, let $D_i$ denote the realized duration at which claimant $i$ becomes reemployed, let $p_{id} \equiv \Pr(D_i = d)$, and define $\lambda_{id} \equiv \Pr(D_i = d \mid D_i > d - 1)$ as the (discrete-time) hazard rate of reemployment.

\textsuperscript{52} Even if reservation wages do not change, contracted wages could still decline as workers approach benefit cuts if there are rents to bargain over and if firms can accurately identify which job applicants are on the verge of benefit reductions. Given the complexity of the benefit calculation—which requires precise information on the exact timing of prior employment spells and UI receipt—this story would place a heavy information burden on potential employers. In Section 7, I find that benefit cuts increase the hazard rate of being recalled to a previous employer—suggesting that benefit cuts affect which job offers claimants choose to accept, rather than (exclusively) lowering wage offers within employment relationships that would have been formed anyway.

\textsuperscript{53} As reported in Appendix Table 2, the peak effect in each specification remains statistically significant at the 5 percent level, with one exception: for women, the wage effects become statistically insignificant when I control for entry-cohort effects, though the point estimates remain negative.
Let $w_i$ denote the log ratio of the reemployment wage to the pre-UI wage (without conditioning on completed duration), and define $\mu_{id} \equiv \mathbb{E}(w_i \mid D_i = d)$ as the average value of $w_i$ among workers who find jobs $d$ months into their spells. I take $p_{id}$, $\lambda_{id}$, and $\mu_{id}$ to be conditioned on $x_{id}$, the explanatory variables used in the hazard and wage equations. I keep the conditioning implicit to simplify the notation.

By the law of iterated expectations, the mean log wage ratio (conditional on being reemployed) equals

$$
\mathbb{E}(w_i \mid D_i \leq 24) = \frac{1}{F_{i,24}} \sum_{d=1}^{24} p_{id} \mu_{id} = \frac{1}{F_{i,24}} \sum_{d=1}^{24} \left( \lambda_{id} \prod_{s=1}^{d-1} (1 - \lambda_{is}) \right) \mu_{id},
$$

where $F_{i,24}$ is the probability of being reemployed within 24 months of UI entry. Holding hazard rates constant, a benefit cut may affect reemployment wages by changing mean wages $\mu_{id}$ at each possible duration. I refer to this as the reservation wage effect. Holding $\mu_{id}$ constant, a benefit cut may affect reemployment wages by raising the hazard rate of reemployment, so that this expression places greater weight on earlier periods when wage offers tend to be higher. I call this the duration effect. A third effect emerges if benefit cuts affect $F_{i,24}$, thereby altering the set of workers for whom I observe post-UI wages. This is the selection effect.

These distinct effects motivate a joint econometric model of reemployment hazards and reemployment wages (as in, for example, Caliendo et al., 2013). I specify the reemployment hazard as in the benchmark specification estimated in Section 5.2. I specify the log wage as $w_{id} = \mu_{id} + \epsilon_{id}$, where $\mu_{id}$ is the fitted value of wages in Equation 6.1 and where I assume $\epsilon_{id} \sim N(0, \sigma_{\epsilon}^2)$. Combining the hazard and wage terms, the likelihood function for claimant $i$ is

$$
L_i(\theta) = \begin{cases} 
\lambda_{iD} \prod_{s=1}^{D-1} (1 - \lambda_{is}) \frac{1}{\sigma_{\epsilon} \sqrt{2\pi}} \exp \left( - \frac{(w_i - \mu_{iD})^2}{2 \sigma_{\epsilon}^2} \right) & \text{if } D_i \leq 24 \\
\prod_{s=1}^{24} (1 - \lambda_{is}) & \text{if } D_i > 24 
\end{cases}
$$

The first case represents workers reemployed $D_i$ months after entering reemployment (so that $w_i \equiv w_{iD}$ by construction). The second case represents workers for whom I don’t observe a post-UI job (or wage) within 24 months. I estimate the model by maximum likelihood.

Adapting the quantification procedure from Section 5.3, I use the fitted model to predict counterfactual reemployment wages in the absence of Hartz IV. First, I denote the fitted (factual and counterfactual) wages at a given duration by $\hat{\mu}_{id}$ and $\hat{\mu}^{cf}_{id}$, where the latter sets the Hartz IV event-time variable $\tau_{id}^H$ to its omitted value, implicitly assuming that Hartz IV lies far enough in the future not to influence wages. Second, let $\hat{\lambda}_{id}$ be the fitted value of the reemployment hazard, and let $\hat{\lambda}^{cf}_{id}$ be the counterfactual hazard when I again set $\tau_{id}^H$ to its omitted value. Using these fitted and counterfactual hazards, I estimate the pdf of completed jobless durations:

$$
\hat{p}_{id} \equiv \hat{\lambda}_{id} \prod_{s=1}^{d-1} (1 - \hat{\lambda}_{is}) \\
\hat{p}^{cf}_{id} \equiv \hat{\lambda}^{cf}_{id} \prod_{s=1}^{d-1} (1 - \hat{\lambda}^{cf}_{is})
$$

31
The corresponding probabilities of being reemployed within 24 months are $\hat{F}_{i,24} = \sum_{d=1}^{24} \hat{p}_{id}$ and $\hat{F}_{cf,i,24} = \sum_{d=1}^{24} \hat{p}_{id}^{cf}$. To simplify notation, I also define the conditional pdf $\hat{q}_{id} \equiv \hat{P}(D_i = d | D_i \leq 24) = \frac{\hat{p}_{id}}{\hat{F}_{i,24}}$, with $\hat{q}_{id}^{cf}$ defined similarly.

Under Hartz IV, the predicted mean wage for worker $i$, conditional on reemployment within 24 months, is $\sum_{d=1}^{24} \hat{q}_{id} \hat{\mu}_{id}$. In expectation, $\sum_{i=1}^{N} \hat{F}_{i,24}$ workers are reemployed, so that each worker’s expected share of the reemployed sample is $\hat{\pi}_i \equiv \frac{\hat{F}_{i,24}}{\sum_{j=1}^{N} \hat{F}_{j,24}}$, with $\hat{\pi}_{i}^{cf}$ defined analogously. The predicted average wage in the full set of reemployed workers is therefore

$$\hat{E}(w) = \sum_{i=1}^{N} \hat{\pi}_i \sum_{d=1}^{24} \hat{q}_{id} \hat{\mu}_{id}, \quad (6.5)$$

with the counterfactual average wage defined similarly. Taking the difference, the predicted effect of Hartz IV is

$$\Delta \hat{E}(w) = \sum_{i=1}^{N} \sum_{d=1}^{24} \left( \hat{\pi}_i \hat{q}_{id} \hat{\mu}_{id} - \hat{\pi}_{i}^{cf} \hat{q}_{id}^{cf} \hat{\mu}_{id}^{cf} \right). \quad (6.6)$$

I can decompose this expression to quantify the three effects described above:

$$\Delta \hat{E}(w) = \sum_{i=1}^{N} \sum_{d=1}^{24} \left( \hat{\pi}_i \hat{q}_{id} \left( \hat{\mu}_{id} - \hat{\mu}_{id}^{cf} \right) \right) + \sum_{i=1}^{N} \sum_{d=1}^{24} \left( \hat{\pi}_{i}^{cf} \hat{q}_{id}^{cf} \left( \hat{\mu}_{id}^{cf} - \hat{\mu}_{id} \right) \right) + \sum_{i=1}^{N} \left( \hat{\pi}_i - \hat{\pi}_{i}^{cf} \right) \sum_{d=1}^{24} \hat{q}_{id} \hat{\mu}_{id} \quad (6.7)$$

The first term shows the wage impact holding jobless durations, as well as the composition of the pool of reemployed workers, constant at their counterfactual values. I interpret it as a reservation wage effect (see Footnote 52). The second term shows the wage impact due to changes in the timing of reemployment, again holding composition constant. The third term reflects selection on observables: it captures any covariance between workers’ predicted wages and the change in their likelihood of being reemployed as a result of Hartz IV. I compute these expressions using all workers entering UI in 2005, the first cohort of UI entrants fully exposed to the new benefit schedule.

Results from this decomposition are reported in Table 4, under the heading “benchmark decomposition”. Overall, I estimate that exposure to Hartz IV reduced the mean initial reemployment wage by 1.6 percent for male claimants and by 2.0 percent for female claimants. The overall impact masks offsetting reservation wage and duration effects: the decline in reemployment wages due to the point-in-time effects of Hartz IV on accepted wages (1.96 percent for men, 2.27 percent for women) is only slightly offset by wage gains from shorter durations (0.27 percent for men, 0.14 percent for women). The selection component is positive but small (0.07 percent for men, 0.12 percent for women). The positive value suggests that the marginal claimants who become reemployed within 24 months as a result of Hartz IV are positively selected on predicted wages. Subtracting this selection term yields an estimated wage effect for a fixed group of workers, namely those who are predicted to have been reemployed in the absence of Hartz IV. For these workers, I estimate the negative causal effect of benefit cuts on average reemployment wages to be 1.7 percent for men and 2.1 percent for women. These losses amount to a sizable share of the wage losses incurred by displaced workers in my sample (which equal, on average, 6.7 percent among male claimants reemployed with 24 months, and 8.8 percent among female claimants).
As usual with Oaxaca-Blinder expressions, this decomposition is not unique: in general, the effect attributed to each channel depends on the order in which the terms are decomposed. One alternative decomposition would weight the reservation wage effects by the factual reemployment shares $\tilde{\pi}_i$ and the factual conditional probabilities $\tilde{q}_{id}$. The alternative formula yields very similar results (presented in the lower panel of Table 4), reflecting the fact that the second-order interactions between the duration and wage effects are small in magnitude.

7 What Kind of Jobs?

To shed additional light on workers’ search behavior and on the mechanisms through which long-term benefit cuts depress initial post-UI wages, this section develops a competing-risks framework to track transitions into different kinds of jobs. I partition jobs in three ways. First, I examine transitions into full-time vs. part-time jobs to gauge whether the negative wage effects found in Section 6 reflect shifts along the intensive margin of labor supply, rather than declines in hourly wages. Net employment gains are mostly driven by full-time jobs, with little change in the part-time share of employment. Second, I distinguish brand-new employment relationships from recalls to the previous employer. I find positive effects on both new-job and recall hazards. The increased recall hazard provides corroborating evidence that benefit cuts worsen workers’ outside options, as my wage results suggest. Third, I broaden the employment concept to encompass low-paid mini-jobs often held during UI receipt. Contrary to received wisdom, the Hartz IV benefit cuts reduced the share of displaced workers who transition into mini-jobs.

7.1 Full time vs. part-time

In Section 6, I found that long-term benefit cuts reduce monthly earnings in a worker’s first regular job after entering UI. Although I have attributed this negative effect to a decline in reservation wages, an alternative possibility is that it reflects a reduction in hours worked rather than a reduction in hourly wages. The IAB data do not report hours worked, but I can explore the intensive margin of labor supply by looking at full-time/part-time status. To answer this question, I distinguish transitions between full-time and part-time regular jobs to bound the component of the earnings response explainable by shifts into part-time employment.

Formally, I adapt my dynamic model to allow for competing risks of accepting a full-time or part-time job. To mirror the wage analysis, I again restrict attention to regular jobs: although some part-time jobs are legally classified as mini-jobs, many others are covered by social insurance and hence fall within the employment concept used in earlier sections. As is standard in competing-risk models, I treat each job type as an absorbing state; that is, I ignore subsequent transitions between job types, such as “promotions” from part-time to full-time jobs. I estimate a separate discrete-time hazard specification for each job type, censoring spells if and when a worker is reemployed into the other kind of job. I use the same explanatory variables as in my benchmark specification.

The lefthand panel of Figure 15 plots the estimated effect of reform-induced long-term benefit cuts on the
competing risks of finding full-time or part-time jobs. Hartz IV has similar, positive proportional effects on the hazard rates of entry into both job types. But cause-specific hazard rates are hard to interpret without the strong assumption that the risks are mutually independent (Heckman and Honoré, 1989). In addition, to assess how shifts between full-time and part-time jobs impact estimated changes in initial post-UI wages, what matters are not the hazard rates, but rather how these hazards translate into the share of workers who end up in each type of job.

I therefore use the fitted model to estimate cumulative incidence functions, which measure the share of workers absorbed into each job type at a given duration since baseline (Fine and Gray, 1999). Let $D_i$ denote completed jobless duration (in continuous time), and let $J_i \in \{\text{full-time job, part-time job, no job}\}$ denote the type of job obtained (with $J_i = \text{no job}$ if $D_i = \infty$). Then the cumulative incidence for job type $j$ at duration $d$ is

$$I_{jd} = P(D_i \leq d \cap J_i = j). \quad (7.1)$$

The overall reemployment rate $F_{id}$ can be expressed as the sum of the type-specific cumulative incidences:

$$F_{id} = \sum_{j \neq \text{no job}} I_{jd}. \quad (7.2)$$

This identity allows me to decompose reemployment rates—or, more to the point, the change in reemployment induced by Hartz IV—into full-time and part-time components.

Adapting the procedure used in Section 5.3, I obtain predicted and counterfactual cumulative incidence functions for each UI claim. I compute the gap between these functions at durations $d \in \{1, 2, \ldots, 24\}$, and I average this gap across all UI claims begun in 2005 as a measure of how Hartz IV affected job types for workers fully exposed to the new benefit schedule. The gap is plotted in the righthand panel of Figure 15. Net employment gains predominantly represent full-time jobs—an unsurprising result given the cross-sectional fact that most regular jobs in my sample are full-time. For men, my estimates imply that the part-time share of employment within the fully exposed 2005 cohort of UI entrants rose slightly, from 4.1 percent to 4.4 percent, as a result of Hartz IV. For women, my estimates imply that the part-time share of employment fell from 30.0 to 29.9 percent.

A bounding exercise suggests that shifts to part-time jobs are unlikely to explain much of the overall wage effect I find. Suppose, for sake of argument, that part-time jobs pay the same hourly wages as full-time jobs but entail only half as many hours. Then, holding hourly wages constant, the positive impact on men’s part-time share would induce only a 0.2 percent decline in mean male earnings—an order of magnitude smaller than the net effect I estimate in Section 6. For women, benefit cuts slightly reduce the part-time share of employment, so that an increased incidence of part-time employment cannot explain my wage results. Though I am unable to rule out

54 The cumulative incidence function is interpretable without assuming independent risks. To understand why, consider an analogy to a randomized controlled trial in which an experimental program assists unemployed workers applying to different kinds of jobs. Under randomized assignment, the net effect of the treatment on reemployment into a given job category at any given duration is identified by a simple difference of means. This is the cumulative incidence. By contrast, without imposing additional structure, the econometrician cannot consistently estimate how the treatment impacted individual search intensity towards each type of job: in the presence of unobserved heterogeneity, observed changes in cause-specific hazards reflect not only average changes in individual transition probabilities, but also compositional changes in the risk set induced by dynamic selection out of unemployment.
shifts in hours within the part-time and full-time job classes, the available evidence suggests that the negative wage effects do not stem from intensive changes in labor supply.

### 7.2 New jobs vs. recalls

A second partition of interest is the distinction between *new jobs* versus *recalls* to the previous employer. Many unemployment spells, especially in cyclical sectors like construction and hospitality, are temporary layoffs that frequently end in recall, and recall expectations are an important determinant of job-search effort (Katz and Meyer, 1990b; Nekoei and Weber, 2015). Although I do not observe workers’ recall expectations at the moment of layoff, I can identify ex post recalls by matching unique establishment identifiers between pre- and post-UI jobs. Insofar as returning to a former employer is “a process not requiring search” (Katz and Meyer, 1990b)—because workers are contacted by their former employer and simply exercise or decline the option to return—the recall hazard offers an additional window into how benefit cuts affect the continuation value of remaining unemployed. Recalls are common in my estimation sample: among claimants reemployed within 24 months of UI entry, 35.8 percent of men and 23.8 percent of women return to their previous employer.

The lefthand panel in Figure 16 plots the effects of Hartz IV on the competing risks of being hired by either a new employer or the previous employer. Both the recall hazard and the new-job hazard display the same hump-shaped effect pattern familiar from the benchmark specification, with the recall hazard rising by about 30 to 35 percent and the new-job hazard rising by about 60 percent in the month of the benefit cut. On face, these results indicate that still-unemployed workers are more likely to find both types of jobs when faced with benefit cuts. Modulo the need to assume independent risks, the increased recall hazard suggests that Hartz IV made workers less selective about the jobs they accept, provide additional confirmation that long-term benefit cuts have worsened the outside option of remaining unemployed.\(^\text{55}\)

Using the cumulative incidence functions, the righthand panel plots the corresponding impact on net transitions into both types of jobs. Consistent with the descriptive fact that recalls typically happen either early in unemployment or not at all, recalls account for a large share of women’s net job gains during the first four months after UI entry but flatline thereafter. Among men, the elevated recall hazard translates into only negligible net gains in recall employment: new jobs account for essentially all of the net employment gains induced by Hartz IV.\(^\text{56}\)

\(^{55}\) If jobs are fully characterized by the wages they offer, then reservation wages are a sufficient statistic for the continuation payoff from staying unemployed. If jobs differ along multiple dimensions, however, workers adopt a more general “reservation utility” cutoff. Although accepting a recall offer is associated with smaller ex post wage losses, some workers may decline even high-paying recall offers if these wage premia reflect a compensating differential for the elevated risk of subsequent layoff in jobs that extend recall offers.

\(^{56}\) This is counterintuitive but entirely possible. Some male claimants are presumably being diverted into new jobs from recalls they would counterfactually have accepted, and these diversions may exactly offset increased recalls among claimants who would counterfactually have found new jobs or remained unemployed.
7.3 Regular jobs vs. mini-jobs

Up to now, I have focused exclusively on jobs subject to social insurance contributions. This employment concept excludes “mini-jobs”, a legally distinct class of low-paid, part-time jobs that are subject to an earnings cap and partly exempt from these contributions.\(^{57}\) In June 2004, mini-jobs accounted for 9.8 percent of aggregate male employment and fully 21.2 percent of female employment.\(^{58}\) Critics of Hartz IV allege that it has fueled the growth of such marginal positions by compelling the unemployed to accept any work they can find, potentially at the expense of job security and other amenities historically provided by the German labor market (Jarosch, 2015; Tazhitdinova, 2016).\(^{59}\) But a priori, reductions in long-term benefits may either promote or deter transitions of UI recipients into mini-jobs. Thanks to the earnings disregard, UI receipt and mini-jobs are not mutually exclusive. The income loss from a benefit cut may induce some claimants to obtain mini-jobs in lieu of pure unemployment, but it also reduces the attractiveness of dual UI receipt/mini-job employment relative to the alternative strategy of seeking socially insured employment.\(^{60}\) Which effect dominates is an empirical question.

I therefore broaden the definition of reemployment to put regular and mini-jobs on the same footing. To focus on transitions that occur after the onset of UI receipt, I drop claimants who hold a mini-job on the date they enter UI (5.0 percent of men and 8.8 percent of women). Figure 17 plots the estimated effects of Hartz IV on the competing risks of entering regular jobs or mini-jobs, together with the effect on the single risk of entering a job of any kind whatsoever. The grey series shows that Hartz IV continues to have large, positive effects on job-finding rates when I expand the employment concept to include mini-jobs. The point estimates are noticeably smaller, however, providing an initial indication that Hartz IV promotes transitions into regular jobs, rather than mini-jobs.

The blue and brown series support this interpretation: whereas transitions into regular jobs become more likely as workers approach reform-induced benefit cuts, transitions into mini-jobs become less likely for men and change little for women.\(^{61}\) As noted previously, however, cause-specific hazards are difficult to interpret without a strong

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\(^{57}\) Workers in mini-jobs are exempt from paying social insurance and income taxes, but employers are still liable for their portion of social insurance contributions. Until 2003, mini-jobs were capped at \(€325\) per month, with a maximum of 15 hours per week. In April 2003, Hartz II eliminated the hours ceiling, raised the earnings ceiling to \(€400\) per month, and allowed workers in regular employment to hold a mini-job on the side without increasing their total tax liability. Hartz II also created a class of intermediate jobs (“midi-jobs”) paying \(€400–800\) per month, with the social security exemption phased out gradually.

\(^{58}\) Own calculation, public-use IAB data. Mini-jobs are even more common after job loss: setting aside workers already holding a mini-job at entry into UI, 17.5 percent of men in my sample and 29.1 percent of women took a mini-job prior to finding regular work.

\(^{59}\) By worsening outside options among displaced workers, benefit cuts may also have contributed to the rise of contingent work arrangements—such as domestic outsourcing, temporary help, and independent contracting—that have been implicated as a proximate source of rising wage inequality in both Europe and the United States (Goldschmidt and Schmieder, 2015; Katz and Krueger, 2016).

\(^{60}\) To see this, consider a static model of sector choice where workers choose among pure unemployment with UI benefit \(b\), benefit receipt coupled with a mini-job at wage \(w_M\) below the earnings disregard, or a regular job at wage \(w_R\). The corresponding payoffs are

\[
\begin{align*}
    u_1 &= b \\
    u_2 &= u(b + w_M) - c_M \\
    u_3 &= u(w_R) - c_R
\end{align*}
\]

where \(c_M\) and \(c_R > c_M\) are the disutilities of marginal and regular work. A benefit cut reduces both \(u_1\) and \(u_2\) while leaving \(u_3\) unchanged; moreover, provided that \(u(\cdot)\) is concave, \(\frac{d}{db}(u_1 - u_2) = u'(b) - u'(b + w_M) > 0\), so that lower benefits reduce the attractiveness of pure unemployment relative to the dual claim-work strategy. The result is that workers switch from strategy 1 to strategies 2 and 3 and from strategy 2 to strategy 3, so that the net effect on mini-jobs is unclear.

\(^{61}\) I omit confidence intervals for visual clarity. The any-job and regular-job hazard effects are precisely estimated and are positive at the 5 percent level for all \(\tau_{id}^H \in \{-8.1\}\) for men and for all \(\tau_{id}^H \in \{-9.2\}\) for women. The mini-job hazard is significantly negative for men from \(\tau_{id}^H = -2\) onward and generally indistinguishable from zero for women (with significant positives for \(\tau_{id}^H = -5\) and \(\tau_{id}^H = 1\)).
independence assumption. Absent independence, the declining mini-job hazard may reflect dynamic selection out of the risk set as some workers enter regular jobs, rather than changes in individual mini-job hazards. Independence is unlikely to hold in this setting: finding rates for regular and mini-jobs may be either complements (if some applicants obtain more job offers across the board) or substitutes (if searching for one type of job crowds out time spent searching for others).

Figure 18 plots the implied effects on the cumulative incidence functions, which are easier to interpret (see Footnote 54). The blue series show that regular jobs more than account for the net employment gains. Contrary to much of the political discourse surrounding the reform, the brown series shows Hartz IV had a modestly negative causal effect on the share of workers drawn into mini-jobs. Among men, these negative causal impacts accrue steadily with duration, reaching 1.4 percentage points two years after UI entry. Among women, absorption into mini-jobs initially rises slightly but then declines, with a net decline of 0.6 percentage points two years after entry. Though not a foregone conclusion, this result is quite plausible, since Hartz IV made it less attractive for workers to claim long-term UI benefits while keeping their earnings low to remain eligible for benefits. These results reveal only the net change in the share of workers who end up in each state; they say nothing about how individual workers’ outcomes were affected. What is clear, however, is that the UI reform reduced net transitions into mini-jobs among former UI claimants, at least at longer durations.

In wage specifications that put regular and mini-jobs on equal footing, I find that long-term benefit cuts actually increase earnings on the initial post-UI job by diverting workers from low-paid mini-jobs. Although this positive effect is in apparent tension with my earlier finding that Hartz IV depressed wages in regular jobs, the sign reversal is unsurprising. Many mini-job holders continue to claim UI, and those who do so can supply only limited hours if they wish to remain below the earnings disregard. Insofar as mini-jobs act as an adjunct to UI for the population I study, my preferred employment concept—which excludes mini-jobs—should better capture how Hartz IV affected earnings potential upon reemployment. Taken together, the patterns in Figures 14, 17 and 18 suggest that Hartz IV induced workers to seek gainful employment in lieu of mini-jobs that are implicitly subsidized by UI—but that it also pushed down wages for those who did so.

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62 The grey series in each panel plots the reform-induced change in overall reemployment rates implied by a single-risk model using a broader employment concept that includes mini-jobs. The reform-induced employment gap peaks about 12 months after entry into UI at 2.4 percentage points for men and 4.1 points for women, with partial convergence thereafter. These magnitudes are about one-third smaller than those obtained when restricting attention to regular jobs (Figure 10).

63 Due to estimation error, the identity in Equation 7.2 holds only approximately in the estimated model. Hence the blue and brown series do not exactly sum to the grey series corresponding to the single-risk model.

64 Consider two possibilities. Hartz IV may have drawn some counterfactual non-workers into regular jobs while also causing some counterfactual mini-job holders to seek regular work. Alternatively, it may have induced some non-workers to obtain mini-jobs but also induced some (larger number of) counterfactual mini-job holders to enter regular jobs. These stories are difficult to disentangle.

65 Although Hartz IV appears to have acted as a brake on transitions from UI into marginal employment, it is quite possible that the broader package of Hartz reforms did foster marginal employment, especially given the mini-job reform of April 2003. My analysis also abstracts from any general equilibrium mechanisms (e.g., market-wide changes in bargaining) through which benefit cuts may have altered the aggregate composition of jobs.

66 Descriptively, I observe considerable bunching of mini-job earnings at the UI disregard of €165 per month. So closely are mini-jobs entwined with the UI system that the tax subsidies accorded to mini-jobs are sometimes likened to an active labor market policy.
8 Conclusion

Many countries offer long-term unemployment assistance for claimants who have exhausted their initial stream of short-term unemployment benefits. Long-term benefits are especially relevant for workers at elevated risk of experiencing lengthy jobless spells—a group of special policy interest, since lengthy spells may depress wages, discourage jobseekers, and lead to permanent exit from the labor force. Despite the prevalence of these two-tiered UI systems, the labor market effects of long-term unemployment benefits are not well understood. Generous long-term benefits—which, in the case of pre-2005 Germany, could potentially replace over half of prior net earnings for an indefinite period of time—may lead to especially long jobless spells by disincentivizing job search, with attendant declines in earnings potential and heavy burdens on public finances. Conversely, they may provide the liquidity needed for displaced workers to engage in efficacious job search, especially when labor demand is slack.

This paper identifies the effects of long-term UI benefit cuts on individual employment, wages, and job characteristics by isolating within-cohort variation in the timing of exposure to those cuts. Using a large sample of UI claimants drawn from administrative records, I find that Germany’s 2005 Hartz IV reform reduced the probability of experiencing a one-year jobless spell by 12.4 percent, with net employment gains concentrated in full-time jobs. Claimants are less likely to transition into low-paid “mini-jobs”, but they receive lower wages in regular jobs, conditional on completed jobless duration. These direct wage losses—which I attribute to declines in reservation wages as workers approach benefit step-downs—are only slightly offset by wage gains due to shorter jobless spells and by observable selection into reemployment. Holding constant the composition of the pool of successful jobseekers, I estimate that being subject to the Hartz IV benefit schedule reduced monthly earnings on a worker’s first socially insured post-UI job by 1.9 percent. These negative impacts suggest that generous long-term benefits may promote productive job search and not merely moral hazard.

Hartz IV was of major policy importance in its own right, and its impact on the German labor market has been heavily debated (e.g., Dustmann et al., 2014; Burda and Seele, 2016). This paper presents the first quasi-experimental evidence on Hartz IV’s direct effects on unemployed workers, a key input into evaluating its overall effect. Taken at face value, estimated increases in individual job-finding can explain roughly a 0.9 percentage point decline in Germany’s steady-state unemployment rate. Although my analysis differences out general equilibrium effects felt by all jobseekers, my partial equilibrium estimates are a useful input into calibrating the market-wide impact of Hartz IV. Gauging the aggregate impact of Hartz IV—including its possible contributions to rising wage inequality (Dustmann et al., 2009), changes in equilibrium job composition (Tazhitdinova, 2016), and the “German employment miracle” of the late 2000s (Burda and Hunt, 2011)—is an important avenue for future research.67

Focusing purely on the steady-state impact of Hartz IV would overlook the population it most immediately affected: the 2.2 million workers already claiming long-term UI in June 2004, who experienced (often steep) benefit

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67 A related literature has debated the macroeconomic impact of UI benefit extensions during the US Great Recession (Hagedorn et al., 2015, 2016; Coglianese, 2015; Chodorow-Reich and Karabarounis, 2016).
cuts overnight if still unemployed on January 1, 2005. Prior to the Hartz era, Germany’s generous safety net had historically been protected by a “reform bottleneck” that stymied efforts to bring long-term replacement rates in line with international practice (Jacobi and Kluve, 2007; Tompson, 2009). Given political gridlock, displaced workers—especially older workers—who entered UI under the pre-reform regime might rationally have expected to claim generous, wage-indexed benefits until entering retirement, effectively becoming labor market participants in name only. Hartz IV presented such claimants with a stark choice: either accept a lower consumption stream or return to the workforce after a long hiatus, under the shadow of stigma or skill depreciation. The causal estimates I estimate encompass both the existing stock of incumbent claimants and subsequent inflows into the UI system. In future work, I plan to further explore how benefit cuts impacted these long-standing UI beneficiaries. At a time of declining labor force participation and lackluster wage growth in the United States and elsewhere, further study of these “long-long-term unemployed” may yield fresh insights about how extended periods out of work affect human capital and earnings potential.

References


Müller, Kai-Uwe, and Viktor Steiner. 2008. “Imposed Benefit Sanctions and the Unemployment-to-
Employment Transition: The German Experience.” IZA discussion paper 3483.


Figures and Tables

Figure 1: Germany’s growing long-term UI caseload in the lead-up to Hartz IV

![Graph showing the growth of long-term UI claimants in Germany](image)

Notes: Aggregate UI caseloads at monthly frequency from Germany’s Federal Employment Agency. Short-term and long-term UI refer to German Arbeitslosengeld and Arbeitslosenhilfe, respectively. I truncate the figure in December 2004 because the 2005 Hartz IV reform replaced the existing long-term UI system with a new benefit regime that encompassed both long-term UI claimants and non-UI welfare recipients (rendering the pre- and post-reform caseloads incomparable).

Figure 2: Pre-/post-UI changes in log monthly earnings by completed jobless duration

![Graph showing changes in log monthly earnings](image)

Notes: Mean difference in log monthly earnings between the job held prior to UI entry and the first social-insurance-covered job obtained after UI entry, binning workers by completed jobless duration. I use a representative 4.7 percent sample of new UI claims initiated by prime-age displaced workers during 2001–2005. I winsorize pre- and post-UI wages at the 0.5th and 99.5th percentiles of the pre-UI wage distribution. Throughout the paper, I deflate wages to 2005 EUR using the consumer price index published by Germany’s Federal Statistical Office.
Figure 3: Potential short-term benefit duration for new UI claims

Notes: Calculation of potential short-term benefit duration for UI claimants in my estimation sample (ages 25–54 at the start of the claim). To establish an initial entitlement, a worker had to be employed in a socially insured job for at least 12 months out of the preceding 3 years. Potential duration is an increasing step function of months worked over the past seven years (with 2 months of benefits accruing for every 4 months of work), up to a maximum value determined by age at entry into UI. The duration ceilings for ages 45+ were lowered in February 2006, but claims already in progress were not subject to this change.

Figure 4: Empirical variation in potential short-term benefit duration

Notes: Observed distribution of potential short-term benefit duration for UI claimants in my estimation sample (rounding up to the nearest month). Mass points at 12, 18, 22, and 26 months correspond to the age-specific ceilings shown in Figure 3. The remaining mass represents claimants who have not maxed out the potential duration attainable given their age. Although a standard UI entitlement lasts at least 6 months, seasonal workers not meeting the usual eligibility criteria are entitled to 3–4 months of short-term benefits if they have paid into the system for at least 6–8 months. In addition, unused benefits are carried forward in the event of a subsequent job loss, so that potential duration (in days) can take on any integer value from 1 to the age-specific ceiling.
Figure 5: Simulated reform-induced changes in potential household income after exhaustion of short-term benefits

Notes: Kernel-smoothed probability distribution functions for the simulated change in potential household income induced by Hartz IV (defined in Equation 2.2). I simulate the change in net income under the 2005 vs. 2004 tax-benefit system for each claimant in my estimation sample by running claimant characteristics through programmatic rules adapted from the OECD Tax-Benefit Model. I measure potential household income just after exhaustion of short-term benefits. See Appendix A for details on the simulation procedure.

Figure 6: Hypothetical benefit changes for pre-reform, interim, and post-reform cohorts

Notes: Hypothetical benefit schedules for successive cohorts of claimants entitled to 12 months of potential benefits. The fall in replacement rate from 60 percent to 53 percent represents a childless claimant who passes the means test for long-term benefits under the pre-reform rules. The sign and magnitude of the subsequent, reform-induced benefit change depend on a complex set of household characteristics and programmatic rules and vary across individuals.
Figure 7: Empirical job-finding hazards among claimants with 12 months of potential short-term benefits

Notes: Raw empirical hazard rates of reemployment among new UI claimants ages 25–44 with exactly 12 months of potential short-term benefits (the maximum possible given their age, as well as the modal potential duration in the estimation sample). The “pre-reform” and “post-reform” cohorts consist of claims beginning in 2001 and 2005, respectively. By construction, all pre-reform spells are completed or censored no later than December 2003, prior to final passage of Hartz IV, and all post-reform spells are immediately subject to the post-Hartz long-term benefit level upon exhaustion of short-term benefits.

Figure 8: Relative job-finding hazards before and after short-term benefit exhaustion

Notes: Estimated proportional effects of short-term benefit exhaustion on transitions to employment using the discrete-time hazard specification of Equations 4.3 and 5.1. Each series plots normalized hazard ratios corresponding to short-term benefit exhaustion for claimants entering UI in either 2001 (pre-reform) or 2005 (post-reform). Control variables are listed in the notes to Figure 9. See discussion in Section 5.1.
Notes: Estimated proportional effects of UI benefit changes on transitions to employment using the discrete-time hazard specification of Equations 4.3 and 4.4. The lefthand panels report normalized hazard ratios corresponding to the main effect of short-term benefit exhaustion (exp(δ_E^k) − 1). The righthand panels report the incremental hazard effect of Hartz IV (exp(δ_H^k) − 1). To allow for duration dependence, aggregate time effects, and seasonality, the model includes a nonparametric baseline hazard, a dummy for each quarter × year interaction, a dummy for each month (not interacted with year), and interactions between four quarter dummies and 3-month duration bins (allowing seasonal effects to vary over the course of an unemployment spell). I also include controls for East German residence, seven age bins, and one-year bins of time worked in the seven years preceding UI receipt (“experience”), as well as interactions between these controls and 3-month duration bins. Finally, I include dummies for deciles of prior wage, three education groups, German nationality, and three household types. Dashed lines represent 95 percent confidence intervals, obtained by clustering on individual. See discussion in Section 5.2.
Figure 10: Path of implied reemployment effects for the fully exposed 2005 UI cohort

Notes: Implied effects of Hartz IV on individual reemployment rates, estimated separately for men and women. I use the benchmark specification from Figure 9 to predict the probability that an individual returns to work within 1–24 months of entering UI under both factual and counterfactual scenarios, where the latter sets the time-to-Hartz variable to the omitted category (≥ 10 months away). I compute the mean gap between these predicted values for the 2005 cohort of UI entrants, who are fully exposed to the post-reform benefit schedule. Dashed lines represent 95 percent confidence intervals, based on 500 draws from the estimated variance-covariance matrix. See Section 5.3 for details.

Figure 11: Robustness of hazard effects to alternative specifications

Notes: Incremental effects of reform-induced benefit cuts on the hazard rate of reemployment. Specification 1 replicates the benchmark estimates from Figure 9. Specification 2 absorbs cohort effects by adding quarter-of-entry fixed effects at quarterly frequency. Specifications 3 and 4 allow the effects of age and experience to differ before/after Hartz IV became salient by including three-way interactions between age/experience, 3-month duration bins, and a post-July 2004 dummy. Specification 4 further adds a full set of interactions between 3-month duration bins and quarter × year effects, letting the shape of the hazard function vary freely over time. Specifications 5 and 6 limit the sample as indicated. The same estimates are presented (with standard errors) in Appendix Table 1. See discussion in Section 5.4.
Notes: Estimated effects of real/placebo UI reforms on the reemployment hazard, using either the actual reform date (January 1, 2005) or a placebo date (January 1 of 2001, 2002, 2003, or 2004). For each specification, I construct a 2 percent sample of new UI claims begun between January 1 of year Y−4 and June 30 of year Y, where Y is the assumed reform year. For the placebo specifications, I recode the time-to-Hartz event-time variable to measure time until the placebo reform. I modify the benchmark specification by censoring ongoing spells as of June 30 of year Y and by pooling all post-event periods into a single coefficient. See discussion in Section 5.5. See Appendix Figure 3 for additional specifications that allow the shape of the hazard rate to vary freely over time.

Notes: Relative job-finding rates for a 2 percent sample of new long-term UI claimants during 2000–2004. I measure duration starting at entry into long-term UI, follow workers until reemployment, and censor incomplete spells 36 months after baseline or at the end of 2006 (whichever is earlier). I assign claimants to terciles of long-term benefit level within cells defined by sex × region × household type × year of entry into long-term UI. I specify the reemployment hazard as a function of quarter effects as well as a full set of quarter × benefit tercile interactions, plus a nonparametric hazard stratified by West/East as well as controls for age bins, German nationality, and household type. The plotted series report normalized hazard effects for the time × benefit tercile interactions.
Figure 14: Benchmark effects of benefit step-downs on the log ratio of post-UI to pre-UI wages

(a) Men

1. Short-term benefit exhaustion
2. Incremental effect of Hartz IV

(b) Women

1. Short-term benefit exhaustion
2. Incremental effect of Hartz IV

Notes: Estimated effects of short-term benefit exhaustion and reform-induced long-term benefit cuts on the log ratio of monthly earnings in the first socially insured job after UI to monthly earnings in the job that preceded entry to UI, from the OLS regression specified in Equation 6.1. I winsorize wages at the 0.5th and 99.5th percentiles of the pre-UI wage distribution within the estimation sample. The explanatory variables are the same as those described in the notes to Figure 9. Dashed lines are 95 percent confidence intervals, clustering on individual. See Section 6.1 for additional details.
Figure 15: Effects of long-term benefit cuts on transitions into full-time vs. part-time jobs

i. Hazard effects

ii. Implied cumulative incidence effects

Notes: Lefthand panel: proportional effects of reform-induced benefit cuts on transition rates into socially insured full-time and part-time jobs, estimated using a competing-risks model that treats each job type as an absorbing state. The explanatory variables match those used in the benchmark hazard specification. Righthand panel: estimated effect of Hartz IV on the cumulative incidence of reemployment into full-time or part-time jobs. To compute these effects, I use the fitted model to predict the cumulative incidence of each reemployment risk for UI claims initiated in 2005 (so that all such claims are “fully exposed” to the reform), with and without the time-to-Hartz IV effect turned on. I then plot the gap between predicted and counterfactual cumulative incidence as a function of months since entry into UI. See Section 7.1 for details.

Figure 16: Effects of long-term benefit cuts on transitions into new jobs vs. recalls

i. Hazard effects

ii. Implied cumulative incidence effects

Notes: Lefthand panel: proportional effects of reform-induced benefit cuts on transition rates into recalls (where the pre-UI and post-UI employer identifiers coincide) or new jobs (where they do not). I include the same explanatory variables used in the benchmark hazard specification. Righthand panel: estimated effects of Hartz IV on the cumulative incidence of reemployment into recalls or new jobs. See Section 7.2 for details.
Figure 17: Effects of long-term benefit cuts on competing risks of regular jobs vs. mini-jobs

Notes: The grey series plot estimated proportional effects of reform-induced benefit cuts on the hazard rate of entry into a job of any kind, using a broader definition of employment that includes low-paid “mini-jobs” alongside regular jobs. The model is otherwise identical to the benchmark specification estimated in Figure 9. The blue and brown series plot proportional effects of reform-induced benefit cuts on cause-specific hazard rates of entry into regular jobs or mini-jobs (excluding spells in which the worker held a mini-job at UI entry). See Section 7.3 for details.

Figure 18: Effects of long-term benefit cuts on net transitions into regular jobs vs. mini-jobs

Notes: The grey series plots the estimated impact of Hartz IV on cumulative reemployment based on a single-risk model in which the employment concept is broadened to include mini-jobs. The blue and brown series plot the estimated effect of Hartz IV on the cumulative incidence of reemployment into regular or mini-jobs. Due to estimation error, the single-risk series does not exactly equal the sum of the cause-specific series. See Section 7.3 for details.
Table 1: Summary statistics for the estimation sample and for a comparison group of employed workers

<table>
<thead>
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<th>Baseline characteristics</th>
<th>A. Estimation sample</th>
<th>B. Comparison group</th>
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<tr>
<td></td>
<td>Men</td>
<td>Women</td>
</tr>
<tr>
<td>Resides in East Germany</td>
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<td>32.7</td>
</tr>
<tr>
<td>Age</td>
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<td>25–34</td>
<td>35.8</td>
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<td>35–44</td>
<td>37.5</td>
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<td>45–54</td>
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<tr>
<td>Married with children</td>
<td>27.3</td>
<td>23.9</td>
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<td>Monthly wage prior to job loss (estimation sample)</td>
<td>2,050.9</td>
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<td>or at quarterly snapshots (comparison group), 2005 EUR</td>
<td>(870.7)</td>
<td>(804.3)</td>
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<td>Initial monthly UI benefit, 2005 EUR</td>
<td>898.2</td>
<td>655.7</td>
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<td></td>
<td>(294.8)</td>
<td>(255.0)</td>
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<tr>
<td>Employed 4+ of last 7 years</td>
<td>74.3</td>
<td>59.5</td>
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<td>Holds mini-job at baseline</td>
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<td>8.8</td>
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<td>Claimant outcomes</td>
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<td>Reemployed into socially insured job within ...</td>
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<td>6 months</td>
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<td>1 year</td>
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<td>2 years</td>
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<td>3 years</td>
<td>82.1</td>
<td>71.9</td>
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<td>Exhausts short-term benefits</td>
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<td>32.7</td>
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<td>Monthly wage upon reemployment, 2005 EUR</td>
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<td>1,465.2</td>
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<tr>
<td></td>
<td>(795.1)</td>
<td>(776.3)</td>
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<td>Obtains mini-job prior to socially insured job (conditional on no mini-job at baseline)</td>
<td>17.5</td>
<td>29.1</td>
</tr>
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<td>Recalled to previous employer</td>
<td>35.8</td>
<td>23.8</td>
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<td>Sample size</td>
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<tr>
<td>Number of UI claims</td>
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<tr>
<td>Number of distinct individuals</td>
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<td>101,037</td>
</tr>
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Notes: The estimation sample consists of prime-age workers (ages 25–54) who initiate a UI claim during 2001–2005 within 30 days of being displaced from a socially insured job. The comparison group is a 2 percent sample of prime-age workers who are employed in socially insured jobs and not claiming UI. To match the temporal and seasonal structure of the estimation sample, I first take quarterly snapshots of these workers on January 1, April 1, July 1, and October 1 for each year 2001–2005. I then reweight each observation so that the weight placed on each quarter equals the share of UI claims originating in that quarter. Household structure is reported in the unemployment register, not in the employment records, and hence cannot be constructed for the comparison group. Values are percentages except where indicated.
Table 2: Effect of long-term benefit cuts on reemployment rates by year of entry into UI

<table>
<thead>
<tr>
<th>Year of UI entry</th>
<th>Month</th>
<th>with reform</th>
<th>w/o reform</th>
<th>Effect of Hartz IV with reform</th>
<th>w/o reform</th>
<th>Effect of Hartz IV</th>
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<td>6</td>
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<td>2004</td>
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<td>75.87</td>
<td>3.35</td>
<td>65.58</td>
<td>59.67</td>
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<td>2.97</td>
<td>70.44</td>
<td>64.70</td>
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</table>

Notes: Implied effects of the Hartz IV reform on the likelihood that an individual is reemployed within 6, 12, 18, or 24 months of claiming UI. To construct the table, I use the benchmark specification to predict the probability that a given jobless spell ends via reemployment at each time horizon. I then repeat this prediction after setting the time-to-Hartz variable to the omitted category (≥ 10 months away). The difference between these predicted values gives the implied effect of Hartz IV on an individual’s reemployment rate. I average these implied effects by year of entry into UI. See Section 5.3 for details.

Table 3: Implied reemployment effects for the 2005 cohort under alternative censoring horizons

<table>
<thead>
<tr>
<th>Censor at 1 year</th>
<th>Censor at 2 years</th>
<th>Censor at 3 years</th>
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</thead>
<tbody>
<tr>
<td>Men</td>
<td></td>
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<tr>
<td>At 12 Months</td>
<td>4.23</td>
<td>3.98</td>
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<td>At 24 Months</td>
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<td>2.86</td>
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<td>At 36 Months</td>
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<td>2.27</td>
</tr>
<tr>
<td>Women</td>
<td></td>
<td></td>
</tr>
<tr>
<td>At 12 Months</td>
<td>6.69</td>
<td>5.86</td>
</tr>
<tr>
<td>At 24 Months</td>
<td>5.74</td>
<td>5.53</td>
</tr>
<tr>
<td>At 36 Months</td>
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<td>4.90</td>
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Notes: Implied effects of Hartz IV on reemployment rates for the fully exposed 2005 cohort, obtained by reestimating the benchmark specification with incomplete spells censored at either 1 year, 2 years, or 3 years (the rest of the paper censors at 2 years). See notes to Table 2 for details.
Table 4: Implied effects of UI reform on mean log reemployment wages for the fully exposed 2005 UI cohort

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**Benchmark decomposition**

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**Alternative decomposition**

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Notes: Estimated effects of reform-induced benefit cuts on log reemployment wages for workers entering UI in 2005 (expressed in log points). The overall effect and its components are computed as in Equation 6.6 and Equation 6.7. The top panel reports results from my benchmark decomposition, which weights the direct wage effects by the estimated counterfactual distribution of jobless duration in the absence of Hartz IV. The lower panel reports results from an alternative decomposition that instead weights these effects by the factual duration distribution. Standard errors in parentheses are based on 500 draws from the estimated variance-covariance matrix.
Supplementary Figures and Tables

Appendix Figure 1: UI-related Google searches spike in the summer of 2004

![Graph showing UI-related Google searches spike in the summer of 2004](image)

Notes: Google Trends data showing the relative frequency of searches for “Arbeitslosengeld” (English: unemployment insurance) and “Hartz IV” within Germany between 2004 and 2006. Each series is rescaled to reach a maximum value of 100 in this period. Google Trends is unavailable prior to 2004.

Appendix Figure 2: Predicted jobless duration by quarter of UI entry

![Graph showing predicted jobless duration by quarter of UI entry](image)

Notes: Mean predicted jobless durations for claimants entering UI in each quarter. The predictions are fitted values from a Weibull model of the reemployment hazard using UI claims that began in 2001. I estimate the model separately by sex × West/East residence. The explanatory variables are seven age bins, one-year bins of time worked in the seven years preceding UI receipt, deciles of prior wage, three education groups, a dummy for German nationality, and three household types. I also control for quarter × year effects but, when forming predictions, set these interactions to the reference category. By construction, temporal variation in each series reflects only compositional changes in the characteristics of new UI claimants.
Appendix Figure 3: Hazard effects of real and placebo UI reforms (time-varying duration effects)

Notes: See notes to Figure 12, which plots the effects of real and placebo UI reforms on the reemployment hazard. This figure augments each specification with a full set of interactions between 3-month duration bins and quarter × year effects. These interactions allow the shape of the hazard rate to vary freely over time. See discussion in Section 5.5.

Appendix Figure 4: Isolating two sources of variation in potential benefit duration (age or prior work history)

Notes: Each specification includes the same controls used in the benchmark specification of Figure 9. The potential duration of short-term benefits depends on both age and experience. Because all workers under 45 are subject to the same age-determined duration ceiling, variation in potential benefit duration within the 25–34 and 35–44 age groups (blue and orange series, respectively) is driven solely by differences in labor force attachment in the 7 years preceding UI entry. The red series instead restricts attention to claimants with the maximum possible benefit duration given their age, so that the variation in duration is driven only by age. For completeness, I also include estimated effects for ages 45–54 (green series), a group for whom benefit duration varies for both reasons.
Appendix Figure 5: Heterogeneity of hazard effects in the month of reform-induced benefit cuts

Notes: Each point corresponds to one of 36 cells defined by sex × West/East residence × three household structures × terciles of the initial short-term UI benefit level. I estimate the benchmark specification separately for each cell; because sample sizes are considerably smaller in this subgroup analysis, I pool the event months \{-9, -8, -7\}, \{-6, -5, -4\}, \{-3, -2, -1\}, \{0\}, \{1, 2, 3\}, and \{4, 5, 6, ...\} to improve precision. For each cell, I plot the estimated hazard effect for the month of the reform-induced benefit cut (\(t_{id} = 0\)) against the mean simulated change in potential post-exhaustion household income, computed as described in Appendix A. Numbers denote benefit terciles. Error bars indicate 95 percent confidence intervals, clustering by individual.

Appendix Figure 6: Robustness of wage effects to alternative specifications

Notes: Estimated effects of reform-induced long-term benefit cuts on the log ratio of monthly earnings in the first socially insured job after UI to monthly earnings in the job that preceded entry to UI, from variants of the regression estimated in Figure 14. See the notes to Figure 11 for a discussion of the control variables and sample restrictions used in each specification. The same estimates are presented (with standard errors) in Appendix Table 2.
Appendix Table 1a: Effects of long-term benefit cuts on reemployment hazards: men

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Quarter-of-entry FEs

- X

Age/experience x post-July 2004

- X

- X

Time-varying duration effects

- X

One UI spell per individual

- X

Enter UI prior to July 2004

- X

Number of UI claims

209,896  209,896  209,896  209,896  143,629  149,587

Number of distinct claimants

143,629  143,629  143,629  143,629  143,629  113,386

Log likelihood

-529,638  -528,891  -529,522  -529,376  -360,816  -375,472

Notes: Each column reports normalized hazard ratios for the periods preceding and following reform-induced benefit cuts, relative to the omitted category of being observed 10 or more months before these cuts occur. See Section 4.3 for a discussion of the benchmark hazard specification, which is replicated in column 1. See Section 5.4 for an explanation of the control variables and sample restrictions used in these specifications. The same estimates are plotted in Figure 11. Standard errors in parentheses are clustered on individual. * p ≤ .10, ** p ≤ .05, *** p ≤ .01.
Appendix Table 1b: Effects of long-term benefit cuts on reemployment hazards: women

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<td>One UI spell per individual</td>
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Notes: See notes to Appendix Table 1a.
Appendix Table 2a: Effects of long-term benefit cuts on log reemployment wages: men

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<td>-0.40</td>
<td>-0.07</td>
<td>-1.55**</td>
<td>-1.12*</td>
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<td>(0.77)</td>
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<tr>
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<td>(0.83)</td>
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<tr>
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<td>-2.44**</td>
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<td>-2.64***</td>
<td>-2.32**</td>
<td>-2.04*</td>
<td>-5.44***</td>
<td>-6.16***</td>
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<td>(1.12)</td>
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<td>-3.25***</td>
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<td>(1.14)</td>
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<td>(1.48)</td>
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<td>(2.95)</td>
</tr>
<tr>
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<td>-3.57**</td>
<td>-2.77*</td>
<td>-8.44***</td>
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<td>(1.62)</td>
<td>(1.86)</td>
<td>(3.18)</td>
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<td>(1.33)</td>
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</table>

Quarter-of-entry FEs
Age/experience x post-July 2004
Time-varying duration effects
One UI spell per individual
Enter UI prior to July 2004

Number of UI claims
162,963  162,963  162,963  162,963  101,460  114,245
Number of distinct claimants
105,282  105,282  105,282  105,282  101,460  82,899
Log likelihood

Notes: Each column reports log wage effects for the periods preceding and following reform-induced benefit cuts, relative to the omitted category of being reemployed 10 or more months before these cuts occur. See Section 6.1 for a discussion of the benchmark wage specification, which is replicated in column 1. The same estimates are plotted in Appendix Figure 6.
Appendix Table 2b: Effects of long-term benefit cuts on log reemployment wages: women

<table>
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<tr>
<th>Months relative to reform-induced benefit cut</th>
<th>(1)</th>
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<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
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<td>Omitted: ≥ 10 months before</td>
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<td>9 months before</td>
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<td>-1.25</td>
<td>-2.13</td>
</tr>
<tr>
<td></td>
<td>(0.98)</td>
<td>(0.98)</td>
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<td>(1.03)</td>
<td>(1.23)</td>
<td>(1.41)</td>
</tr>
<tr>
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<td>-0.69</td>
<td>0.87</td>
<td>0.07</td>
<td>-2.47</td>
</tr>
<tr>
<td></td>
<td>(1.07)</td>
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<td>(1.23)</td>
<td>(1.32)</td>
<td>(1.57)</td>
</tr>
<tr>
<td>7 months before</td>
<td>-0.06</td>
<td>1.44</td>
<td>-0.71</td>
<td>0.83</td>
<td>0.08</td>
<td>-1.35</td>
</tr>
<tr>
<td></td>
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<td>(1.17)</td>
<td>(1.26)</td>
<td>(1.30)</td>
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</tr>
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<td>-0.25</td>
<td>-1.26</td>
</tr>
<tr>
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<td>(1.40)</td>
<td>(1.42)</td>
<td>(1.46)</td>
<td>(2.17)</td>
</tr>
<tr>
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<td>1.03</td>
<td>0.23</td>
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</tr>
<tr>
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<td>(1.31)</td>
<td>(1.46)</td>
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<td>(2.27)</td>
</tr>
<tr>
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<td>0.32</td>
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<td>(1.55)</td>
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<td>(2.62)</td>
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<td>(1.66)</td>
<td>(1.69)</td>
<td>(1.76)</td>
<td>(2.87)</td>
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<td>(1.69)</td>
<td>(1.71)</td>
<td>(1.82)</td>
<td>(3.11)</td>
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<tr>
<td>Month of change</td>
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<td>-3.96**</td>
<td>-3.56**</td>
<td>-5.46***</td>
<td>-12.36***</td>
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<tr>
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<td>(1.70)</td>
<td>(1.74)</td>
<td>(1.76)</td>
<td>(3.40)</td>
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<tr>
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<td>-3.61*</td>
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<td>-5.84</td>
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<td>(3.73)</td>
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<td>(2.41)</td>
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<td>(4.62)</td>
</tr>
<tr>
<td>4+ months after</td>
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<td>(1.72)</td>
<td>(1.96)</td>
<td>(1.49)</td>
<td>(3.37)</td>
</tr>
</tbody>
</table>

Quarter-of-entry FEs                                                                 X
Age/experience x post-July 2004                                                                 X X
Time-varying duration effects                                                                 X
One UI spell per individual                                                                 X
Enter UI prior to July 2004                                                                 X

Number of UI claims                                                                 83,716 83,716 83,716 83,716 61,218 59,048
Number of distinct claimants                                                           63,884 63,884 63,884 63,884 61,218 48,575

Notes: See notes to Appendix Table 2a.
### Appendix Table 3: Cross-validation of imputed spousal earnings

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<th>(3)</th>
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<th>(5)</th>
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<td>2421.70***</td>
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<td>(15.04)</td>
<td>(15.91)</td>
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<td></td>
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<tr>
<td>1{Spouse is male} x 1{West Germany}</td>
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<td></td>
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<tr>
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<td>Claimant's monthly earnings</td>
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<td>0.38***</td>
<td>0.38***</td>
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<td>(0.01)</td>
<td>(0.01)</td>
<td>(0.02)</td>
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</tr>
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<td>(17.02)</td>
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<td>Number of additional children</td>
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<td>148.54***</td>
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<td>(18.94)</td>
<td>(51.82)</td>
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<tr>
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<td><strong>Yes</strong></td>
<td><strong>Yes</strong></td>
<td><strong>Yes</strong></td>
<td><strong>Yes</strong></td>
</tr>
<tr>
<td><strong>Sex of claimant</strong></td>
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<td><strong>Pooled</strong></td>
<td><strong>Pooled</strong></td>
<td><strong>Male</strong></td>
<td><strong>Female</strong></td>
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<td>Number of distinct claimants</td>
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<td>127,094</td>
<td>127,094</td>
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<td>R-Squared</td>
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<td>0.04</td>
<td>0.14</td>
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</table>

Notes: Results from OLS regressions of imputed monthly spousal earnings on claimant characteristics. All specifications control for calendar-year dummies, region (West or East), seven age bins, and German nationality. Robust standard errors in parentheses are clustered on individual. See Appendix A for details.
Appendix A  Details on the Tax-Benefit Simulations

This appendix describes the methodology I use to simulate changes in the net long-term replacement rate induced by Hartz IV (as discussed in Section 2). My simulations are adapted from the publicly available Stata programs provided by the OECD Tax-Benefit Model (downloadable at http://www.oecd.org/els/soc/Models.zip).

The IAB data report many of the key inputs into the tax and benefit calculations, including gross earnings, region, marital status, and the number of dependent children (along with the age of the youngest child). Another critical input—spousal earnings—is not directly available, but I can back out a proxy for spousal earnings from a claimant’s implied income tax liability. Since I do not observe assets, I follow the OECD default and assume that households have negligible assets, so that the asset means test for long-term UI receipt binds under neither pre- nor post-reform rules. Additional assumptions are needed for rental expenses, the ages of older children, and other inputs. I assume that households apply for any public benefits for which they are eligible, including unemployment assistance, housing benefits, and childcare benefits.

The basic simulation procedure is as follows:

1. Impute missing data inputs, notably spousal earnings.
3. Calculate the potential change in post-exhaustion household income defined in Equation 2.2.

Income taxation and social insurance contributions

Federal income taxes are levied on gross household earnings, after deductions for social security contributions, work-related expenses, and special expenses. Married couples typically file jointly, and tax calculations employ the so-called “income splitting” method, under which gross earnings are assigned equally to each spouse for purposes of computing tax liability. Households with dependent children may claim either a child tax allowance or a child tax credit; personnel in the federal tax office make this choice on the household’s behalf, acting to minimize the household’s tax liability. Following the OECD, I therefore select the tax-minimizing option. Tax liability is computed as a piecewise quadratic function of taxable income, plus an additional “solidarity surcharge” levied on earnings above a statutory exemption. There are no state or local income taxes.

In addition to income taxes, individuals earning above the mini-job threshold are subject to mandatory social insurance contributions. In 2004, workers owed 9.75 percent of gross earnings for pension contributions, 3.25 percent for UI, 7.00 percent for health insurance, and 0.85 percent for long-term care insurance, up to ceilings that are seldom reached by claimants in my sample. Employers make equal-sized contributions, so that payroll taxes amount to roughly 40 percent of gross earnings. Note that, unlike the US system, the German UI system does not experience-rate firms when assessing their UI contributions.

Imputation of spousal earnings

Spousal earnings are a key determinant of long-term UI eligibility, but spouses are not linked in my data. I circumvent this problem by inferring spousal income from a claimant’s implied income tax liability prior to job loss. The initial monthly short-term UI benefit is calculated as

\[ \text{UI benefit} = RR \times (\text{gross monthly earnings} - \text{income tax liability} - \text{social insurance contributions}). \]  

I observe the initial UI benefit level as well as gross monthly earnings prior to UI receipt. As described above, social insurance contributions are a simple function of earnings. The replacement rate (RR) equals 60 or 67 percent, depending on whether the claimant has dependent children (also observed). I can therefore express a claimant’s income tax liability as a function of observables.

---

68 I have also adapted Stata code from the TAXBENEXTRACT package published by Alexandre Desbuquois. Information in this section is taken from the German Social Code, the publication series Social Security at a Glance published by the German Federal Ministry of Labour and Social Affairs, and the annual OECD publication Taxing Wages (particularly the 2004 and 2005 editions).

69 Mini-jobs are exempted from worker-side social insurance contributions; employers, however, are subject to an omnibus 25 percent contribution rate for mini-jobs. Effective April 1, 2003, the Hartz II reform created an additional class of intermediate (“midi”) jobs with gross monthly earnings between €400–800. These jobs pay partial social insurance contributions at an intermediate rate.

70 Goldschmidt et al. (2014) match cohabiting spouses in the IAB based on surnames and residential addresses, but the match rate is fairly low and the matched sample is not fully representative.
Next, I exploit the German tax system’s reliance on full income splitting for married couples. That is,
\[
\text{income tax liability} = f(0.5 \times [\text{own gross earnings} + \text{spousal earnings} + \text{unearned income}])
\]  
(A.2)
Given income tax liability and the claimant’s own gross earnings, I can back out the sum of spousal earnings and unearned income (such as dividends). For simplicity, I refer to this measure as a proxy for spousal earnings.
In Appendix Table 3, I cross-validate this proxy by testing a number of propositions one would expect to hold for a measure of spousal earnings:

1. Imputed spousal earnings are much higher for female claimants (with male spouses) than male claimants.\(^{71}\)
2. The gender gap in spousal earnings is larger in West Germany, where fewer women are employed.
3. Pre-UI claimant earnings are positively correlated with imputed spousal earnings, as expected given some degree of assortative matching between husbands and wives.
4. Among male claimants (with female spouses), spousal earnings are decreasing in the number of children present, with an additional drop if the youngest child is under age 5.
5. Calculating “spousal earnings” for single claimants yields much smaller numbers (not shown in table).
These checks provide some reassurance that—though imperfect—my proxy for spousal earnings contains meaningful information. I use this proxy when simulating income changes for married claimants.

**Benefits under the 2004 rules**

**Unemployment benefits**

Upon job loss, individuals who have established a UI entitlement can register as unemployed and begin receiving benefits. Short-term unemployment benefits (Arbeitslosengeld) replace a fixed fraction of a claimant’s prior earnings, net of income and payroll taxes. For dual-earning couples, I assign income taxes in proportion to each spouse’s gross earnings. The reference net earnings used in the UI calculation are capped at €61,440 (annual), which almost never binds in my data. The net replacement rate is 60 percent for childless claimants and 67 percent for claimants with at least one dependent child. Benefits are paid for 30 days out of each month, and checks are issued at the end of the month. Unemployment benefits are not taxed.
The earnings disregard is the larger of €165 or 20 percent of the full benefit amount; earnings above this threshold reduce UI benefits one-for-one. Workers are also limited to 15 hours of work per week. In all simulations, I assume that claimants have zero earned income while unemployed.\(^{72}\)

Upon exhausting short-term benefits, UI claimants could apply for tax-financed, means-tested long-term unemployment assistance (Arbeitslosenhilfe). Long-term benefits replaced up to 53 percent of prior net earnings for childless claimants and 57 percent for claimants with one or more children. Benefits are reduced one-for-one for earnings above the UI earnings disregard, as well as for alternate sources of income such as rentals. Furthermore, benefits are reduced one-for-one for spousal earnings net of taxes, work-related expenses, and a spousal earnings exemption.\(^{73}\) Finally, long-term UI benefits cannot be claimed unless (or until) household assets fall below an asset exemption. Prior to 2003, the asset exemption equaled €520 times the sum of the claimant’s age and (if married) spouse’s age, up to a maximum of €33,800. Under Article 11 of Hartz I, the asset limit fell to €200 per year of age for long-term UI claims initiated after January 1, 2003.\(^{74}\) Prior to Hartz IV, workers who continued to satisfy the means test (conducted once annually for continuing claims) could claim long-term UI benefits indefinitely until reaching retirement age.

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\(^{71}\) German law did not permit same-sex couples to file their taxes jointly until 2013. Marital status in the IAB data pertains to husband-wife pairs, since same-sex marriage has not been legalized.

\(^{72}\) Hartz IV reduced the earnings disregard for some workers by eliminating the 20 percent minimum disregard. After the reform, everyone’s disregard is capped at €165 per month.

\(^{73}\) The exemption level equals the larger of a legislated minimum or the spouse’s hypothetical long-term replacement rate in the event of unemployment. The legislated minimum has changed over time. Prior to 2003, the minimum exemption equaled €603 per month, corresponding to the “subsistence level” recognized by German law, plus a supplemental exemption of €151. In 2003, the minimum exemption fell to €482, which is 80 percent of the subsistence level, and the supplemental exemption was eliminated. The subsistence level itself was increased in 2004, so that the minimum spousal exemption stood at €511 per month in 2004.

\(^{74}\) The higher asset limit remained in place for workers born before 1948, all of whom fall outside my sample.
Other benefits

Regardless of their employment status, sufficiently poor households could apply for additional means-tested welfare benefits, known as social assistance (Sozialhilfe). Social assistance is calculated by computing each household’s assessed need—the sum of individual allowances and housing/heat allowances—and deducting net income above a modest earnings disregard.

Individual allowances were revised annually and varied somewhat across municipalities. For 2004, the OECD reports that the base allowance paid to household heads averaged €295 in the West and €285 in the East; additional payments were made for dependent spouses (80 percent of the base rate) and children (50–90 percent depending on age), with extra assistance available to single parents. Households applying for social assistance are reimbursed for all “reasonable” housing and heating expenditures. For simplicity, I set household rents (and corresponding benefit ceilings) based on benchmark values for Berlin in 2005.

Once household need has been computed, social assistance is means tested on the basis of household income—specifically, gross earnings (net of a disregard) plus unemployment benefits plus alimony minus income and payroll taxes. The level of social assistance then equals

\[
\text{social assistance} = \max(0, \text{assessed need} - \text{household income net of means testing}).
\]  

(A.3)

In addition to social assistance, German households could apply for means-tested housing benefits (Wohngeld) to assist either with rent or mortgages/home maintenance. The benefit is a complex formula that depends both on household income and on rental expenditures, up to a ceiling that depends on household size and the municipality’s rent level. I follow the OECD in assigning the highest rent level (VI) to all households, regardless of where they reside.

Benefits under the 2005 rules

Hartz IV replaced long-term unemployment assistance and social assistance with a single, means-tested benefit (Arbeitslosengeld II). Rather than being indexed to prior wages, the new benefit level is determined by the household’s assessed living requirements. The core benefits consist of (i) lump-sum payments for basic living expenses, (ii) assistance with housing and utility expenses, and (iii) a temporary supplement to cushion the transition to the new benefit schedule.

In 2005, the standard monthly allowance equaled €345 in the West and €331 in the East. Singles receive 100 percent of the standard allowance; married couples receive 90 percent per spouse. The benefit payment is increased by 60 percent of the standard allowance for each dependent child up to age 14 and by 80 percent of the standard allowance for children 15 or older. The new benefit system also provides for housing and heating expenses. The level of housing assistance is not explicitly stated in the law; rather, municipalities are instructed to cover “reasonable” accommodation and heating costs. I use OECD estimates of housing and heating allowances for Berlin residents.

To ease the transition to the new system, the new benefit system includes a temporary supplement for long-term UI recipients who previously exhausted short-term benefits. The supplement depends on the difference between the value of short-term benefits on the eve of benefit exhaustion and the value of long-term benefits thereafter. In the first year after short-term exhaustion, the supplement equals \(\frac{2}{3}\) of the assessed difference, up to a ceiling that depends on household structure (equaling €160 for singles and €320 for couples, plus €60 for each child). The supplemental payment and the payment ceiling decline are cut in half after one year and expire completely after two years. In my benefit simulations, I compute net replacement rates just after short-term benefit exhaustion, when the supplement is maximized.

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75 Single parents may claim alimony for up to 72 months for each child under 12. I follow the OECD in applying the alimony rate for children under age 6, which in 2004 equals €122 per child per month.

76 I depart from the OECD’s calculation in that I include housing/heating allowances in the post-reform benefit level for this calculation. Informational materials published by the Federal Employment Agency during the mid-2000s state explicitly that these allowances belong in the supplement formula.

77 The two-year time limit begins on the date of short-term benefit exhaustion, even if exhaustion predated the onset of Hartz IV. For example, a claimant who exhausted short-term benefits on December 31, 2002 would not be eligible for the supplement even if she were still unemployed in 2005. A claimant who exhausted short-term benefits on June 30, 2003 would be eligible for a one-half supplement from January 1, 2005 through June 30, 2005 if unemployed during that period. For simplicity, when imputing the immediate benefit change experienced by a claimant at time \(t_H\), I compute the supplement as though the claimant were at the beginning of the two-year grace period. This is a conservative assumption that maximizes the value of the temporary supplement (and hence the generosity of the post-reform system).
Appendix B  Further Discussion of the Placebo Exercise

In Section 5.5, I conduct a falsification exercise to test whether job-finding hazards respond to placebo long-term benefit reforms assumed to take effect on January 1 of 2001, 2002, 2003, or 2004. Although the placebo estimates for 2001, 2002, and 2003 are consistently close to zero, I do estimate modest, but statistically significant, increases in the reemployment hazard as workers approach the January 2004 placebo reform (see Figure 12).

What accounts for the 2004 placebo effects? One possibility is that, despite truncating the 2004 placebo analysis in June 2004, I may nonetheless be picking up anticipation of Hartz IV itself. Although final passage of Hartz IV did not occur until July of that year, the law passed the lower house of parliament in December 2003. If UI claimants were sufficiently attentive, patient, and concerned about the scarring effects of long-term unemployment, they might rationally have begun reacting to Hartz IV far in advance of the actual implementation date. The degree of anticipation in the benchmark estimates of Figure 9—with significant increases in job-finding emerging as early as eight or nine months prior to benefit cuts—renders this conjecture plausible.

A second possibility is that I may be detecting lagged effects from stricter asset means-testing rules that apply to long-term UI claims initiated after January 1, 2003. The IAB data do not report assets, but I observe a small decline in the share of short-term UI exhaustees who transition to long-term UI after this date, suggesting that the new asset limits did bind for some claimants who would previously have been eligible. In unreported specifications, I find that interacting the main effect of benefit exhaustion \( \tau_{H} \) with a dummy for whether exhaustion is projected to occur under the tighter asset rules has little impact on the 2004 placebo effects, casting some doubt on this explanation. Adding these additional interactions to my benchmark specification also has little impact on my estimated Hartz IV effects.

A third possibility is that the placebo effects may stem from an increase in benefit sanctions that occurred in mid-to-late 2003. The Hartz I reform of January 1, 2003 narrowed the grounds on which claimants could turn down job offers without incurring sanctions, and an internal memorandum circulated by the Federal Employment Agency in April 2003 instructed caseworkers to apply sanctions more vigorously (Müller and Steiner, 2008). Nationwide, the number of sanctions imposed each month for refusing suitable job offers tripled over the course of 2003, peaking in April 2003 and then declined by half between September 2003 and the end of 2004. To my knowledge, the risk of being sanctioned during this period was not directly related to time remaining until benefit exhaustion. But a spurious correlation between sanctions and the placebo-reform event-time variable could potentially arise if the risk of being sanctioned during this period was not directly related to time remaining until benefit exhaustion. To address this possibility, Appendix Figure 3 augments the placebo specifications with a full set of interactions between 3-month duration bins and quarter × year effects, to absorb any temporal changes in job-finding that are linked to duration since entry as distinct from duration until benefit cuts. Adding these controls strengthens the 2005 causal effects while attenuating the 2004 placebo effects, especially for women. In this specification—which most stringently isolates variation related to remaining benefit duration—the effects of the 2005 reform consistently lie above those of the 2004 pseudo-reform, and the peak effect is three times larger for both men and women.

It is difficult to adjudicate among competing explanations for the modest, but positive, 2004 placebo effect. Nonetheless, the placebo estimates weigh against a secular trend towards a stronger exhaustion spike, and they reveal much stronger responses to the true 2005 reform than to the earlier placebos.

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78 The scope for anticipatory effects is strongest for high values of \( \tau_{H}^{2004} \), which are closest in time to the onset of Hartz IV. For example, the positive placebo effects for \( \tau_{H}^{2004} \geq 0 \) observed among female claimants are identified by women observed in the first half of 2004, after Hartz IV was passed by the Bundestag.

79 These sanction counts are taken from aggregate data published by the Federal Employment Agency. Sanctions are difficult to identify in the IAB microdata during my sample period. However, there is considerable geographic variation in the degree to which sanctions rose in mid-2003, stemming both from local labor market conditions and from the policy preferences of the local employment offices (Müller and Oschmiansky, 2006; Müller and Steiner, 2008). I am working to obtain local sanction rates, so as to gauge whether the 2004 placebo effect is stronger in areas with elevated sanction rates. An advantage of this local approach is that it captures not only the risk of being sanctioned varies with duration since entry into UI. This is quite possible, since the set of jobs that a claimant is expected to accept broadens with time spent out of work.

80 Exploiting the discontinuity in potential benefit duration induced by the increased duration ceiling at age 45, Schmieder and Trenkle (2016) show that—at least during their 2008–2010 sample period—sanctioning probabilities over the course of an unemployment spell do not vary with baseline potential benefit duration, suggesting that caseworkers do not disproportionately sanction claimants who are close to benefit exhaustion.

81 Caseworkers assess the suitability of a job offer in light of its compensation relative to prior earnings, alongside other criteria like commuting time and marital status. According to Ebbinghaus and Eichhorst (2009), “During the first three months, the unemployed can reject jobs offering less than 80 percent of prior earnings, thereafter less than 70 percent until the sixth month of unemployment, and finally after six months of unemployment, all jobseekers have to accept jobs providing net earnings equal to or higher than unemployment insurance benefits.” Hartz I transferred the burden of proof for declining an “unsuitable” job offer from the caseworker to the claimant.